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OTHER WORLDS THAN OURS

THE PLURALITY OF WORLDS STUDIED UNDER THE LIGHT OF RECENT SCIENTIFIC RESEARCHES

BY

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PREFACE.

THE GENERAL PURPOSE I have had in view in writing the present treatise will be gathered from the introductory pages; but I wish to offer here a few remarks on certain points of detail.

It will be seen that, on many of the subjects dealt with in this work, I have propounded views which differ from those usually accepted. I have not done this from any love of novelty, nor from any desire to attract attention by *bizarre* or fanciful theories. Each of the new views here presented has been the result of a careful study of the subject dealt with, and I have searched as anxiously for considerations opposed to any novel theory as for arguments in its favor. If others should be more successful than I have been in finding reasons for rejecting any of my views, I shall be ready to abandon them without regret. I trust I am free from that weakness which forces a man to regard every theory he has once advocated as a matter to be defended at all hazards. No weakness more mischievously affects the work of the student of science. As Faraday said, "Truth should be the primary object of the philosopher;" and this can never be the case if, where he imagines he holds a theory, the theory has in truth possession of *him*.

Some among my readers will recognize, in the views here presented, the growth of ideas which I have dealt with consecutively, with more or less fulness, in the pages of several quarterly, monthly, and weekly serials, and in one of our leading daily newspapers. I refer to this, because it has happened to me several times lately to be accused of plagiarism, when I have had occasion, in developing fresh ideas on a subject, to repeat statements which (unknown of course to my accusers) had proceeded from my own pen. It is not often one is accused of stealing one's own ideas, but that is a pleasure I have more

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than once been enabled to enjoy of late, and I here present my compliments to those who (anonymously or otherwise) have afforded me that luxury.

Wherever it has been in accordance with the custom of any journal, however, I have always written under my own name.

Since the manuscript of this work was placed in the printers' hands, I have obtained fresh evidence on some of the theories dealt with in the following pages.

One of the most surprising phenomena ever witnessed by the telescopist — a phenomenon I had read of long since, but had not thought of in connection with my subject — seems to me to afford stronger evidence than any adduced in the text, in favor of my theory that the major planets are subsidiary suns supplying heat (if not a minute proportion of light even) to their satellites. I refer to the observation made by Admiral Smyth, that on one occasion the second satellite of Jupiter, twelve minutes after entering on the disk of the planet, was seen *outside the limb,* "where it remained four minutes, and then suddenly vanished." Two other equally competent observers, Maclear and Pearson, witnessed the same phenomenon. "Here," says Webb, "explanation is set at defiance." But it is precisely where explanation seems set at defiance, that the true student of Nature is most hopeful of gaining instruction. The observation is very startling, it is true; and the explanation may be expected to be also surprising. But I think it is not far to seek. The satellite cannot have retraced its course; Jupiter cannot have shifted his place; our atmosphere cannot be in question: surely, when all these explanations are eliminated, our task is rendered easier instead of more difficult. A change of shape in Jupiter, corresponding to that which I have endeavored to exhibit as explaining Saturn's occasional assumption of the square-shouldered aspect, would obviously account for the phenomenon. We know that Schröter suspected an apparent flattening of portions of Jupiter's outline. Here we have an effective confirmation of that long-doubted observation. If we consider the matter rightly, the observation made simultaneously by Smyth, Maclear, and Pearson, makes that view all but certain, which in the text I have presented only as a highly-probable hypothesis.

In preparing the Maps for my new Atlas (now nearly ready), I have detected signs of systematic aggregation among stars visible to the naked eye, which seem to me to place beyond all question the fact that Sir William Herschel adopted an erroneous hypothesis as the basis of his system of star-gauging. The fact that about one-third of the lucid stars are collected in a region having the greater Magellanic Cloud nearly in its centre, and covering less than one-sixth of the heavens, has never yet, so far as I am aware, been noticed. Supplemented by other facts detected during the work of transferring the stars of the British Association Catalogue to my Maps, the existence of this rich region around the Nubeculæ disposes at once of the hypothesis of a generally uniform distribution within the sidereal system.

I may add in this place that it is not the case, as has been recently asserted, that my theories respecting the sidereal system have been founded on the discovery that certain nebulæ are gaseous. That discovery, so far from being opposed to the theories of Sir William Herschel, afforded most striking evidence of his wonderful reasoning powers, since he had been led to express his firm conviction that many nebulæ are gaseous, had confidently asserted that the Orion nebula is so, and had even anticipated the discovery of the variability of the irregular nebulæ, recently effected by Le Sueur of Melbourne.

My theory respecting the sidereal system has been based on the signs of systematic aggregation among the lucid stars, and of a more intimate association of those stars with the Milky Way than could be expected were Sir William Herschel's fundamental theory correct. My first paper on the subject was entitled "Notes on Star-Streams"; and it was only while inquiring into the nature of stellar aggregation that I was led to notice the laws of nebular distribution, and so to inquire into the relations between stars and nebulæ.

The theory brought forward in the chapter on Meteors and Comets is not altogether new. The general idea on which it is grounded has been dealt with by Mayer and Thompson, while the relation between the motions of discrete bodies and the formation of systems of orbs has been dealt with by Sir John Herschel, in considering his father's hypotheses respecting the nebulæ. That idea, however, presented itself independently to my mind when I was writing my treatise on Saturn (at which time my acquaintance with scientific literature was very limited indeed), and is definitely stated in Note B of the Appendix to that work. The line of reasoning is wholly new, I believe, by which I have endeavored to show that those peculiarities of the solar system which have hitherto been regarded as affording the strongest objection to the hypothesis of development, may be regarded as in reality the direct result of the processes by which the solar system has reached its present condition. In the preface to my treatise on Saturn I touched on the possibility that some such explanation of those peculiarities might be found, remarking that in the rings of Saturn astronomers may one day recognize the action of the processes by which the solar system has attained its present state.

In the chapter on the Sun I have entered at some length into the subject of the solar corona, partly because that subject is full of interest in view of the approaching total solar eclipse visible in the south of Europe, and partly because I have seen with regret that an erroneous theory of the corona has been recently promulgated, which seems likely at the present conjuncture to affect mischievously the progress of research into this interesting question of solar physics. I have heard with much pleasure that the Astronomer Royal, at the last meeting of the Astronomical Society, altogether repudiated any share in starting this theory. Although I had seen his name associated with it, I had always thought it incredible that a mathematician so skilful and clear-sighted should have advanced or adopted so ill-considered an hypothesis.

I tender my best thanks to Mr. Browning, F. R. A. S., for the beautiful colored pictures of Jupiter and Saturn, which illustrate the chapters on those planets; and to Mr. Brothers, F. R. A. S., for his careful revision of the proof-sheets, and the detection of more than one error which had escaped my scrutiny.

RICHARD A. PROCTOR

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OTHER WORLDS THAN OURS.

INTRODUCTION.

ASTRONOMY and GEOLOGY owe much of their charm to the fact that they suggest thoughts of other forms of life than those with which we are familiar. Geology teaches us of days when this earth was peopled with strange creatures such as now are not found upon its surface. We turn our thoughts to the epochs when those monsters throve and multiplied, and picture to ourselves the appearance which our earth then presented. Strange forms of vegetation clothe the scene which the mind's eye dwells upon. The air is heavily laden with moisture to nourish the abundant flora; hideous reptiles crawl over their slimy domain, battling with each other or with the denizens of the forest; huge bat-like creatures sweep through the dusky twilight which constituted the primeval day; weird monsters pursue their prey amid the ocean-depths: and we forget, as we dwell upon the strange forms which existed in those long-past ages, that the scene now presented by the earth is no less wonderful, and that the records of our time may perhaps seem one day as perplexing as we now find those of the geological eras.

Astronomy has a kindred charm. We cannot indeed examine the actual substance of living creatures existing upon other celestial bodies; we cannot even picture to ourselves their appearance or qualities; and only in a few instances can we even form any conception of the conditions under which they live. But we see proofs on all sides that, besides the world on which we live, other worlds exist as well cared for and as nobly planned. Nay, we see globes by the side of which our earth would seem but as a tiny speck; we trace these globes as they sweep with stately motion on their appointed courses; we watch the return of day on the broad expanse of their surface; and we see systems of satellites which are suspended as lights for their nocturnal skies. We further find that our sun is matched by a thousand thousand suns amid the immeasurable depths of space; and the mind's eye pictures other worlds like those which course around the sun, travelling in stately orbits around his fellow-luminaries.

Long, however, before the wonders of modern astronomy had been revealed to us, men of inquiring minds seem to have been led, as by an irresistible instinct, to examine into the resemblance which may exist between our world and other worlds surrounding it on every hand. It has not been the mere fanciful theorizer who has discussed such questions, but men of the highest eminence in science. In long-past ages Anaximander and Pythagoras studied the subject of other worlds than ours; later, such men as Huyghens, Galileo, and Newton, have dwelt upon the same interesting theme; while, in our own day, Whewell and Brewster have employed their scientific and dialectic skill in defending rival theories upon the subject.

Undoubtedly a large share of the interest with which the question of other worlds than ours has been regarded, is due to the fact that, as the science of astronomy has progressed, the subject has continually presented itself under new aspects. The question, in fact, is one of those which are ever new and ever old. It has all the charm belonging to subjects which men in all ages have delighted to discuss, while it is associated in the most intimate manner with the progress of modern science. With what a charm of novelty, for instance, the discussion between Whewell and Brewster invested the subject! No doubt a large portion of that charm was due to the personal qualities of the two disputants. Yet, despite the skill with which each of them presented the arguments belonging to his own side of the controversy, few could have read with any interest a discussion on a subject so well worn, had it not been that the arguments were drawn from the discoveries which had recently been made by astronomers. Nor was it uninteresting to notice how these discoveries at once seemed to acquire a new interest when they were associated with the subject of life in other worlds. Facts which had attracted little notice at the time of their discovery, at once assumed importance, when it was seen how they bore on the rival views which Whewell and Brewster were enforcing. The interest with which the public regard many of these discoveries may, indeed, be said to date from the controversy between those eminent men.

No very long interval, if we count by years, has elapsed since the "Plurality of Worlds" and "More Worlds than One" were written. Yet so rapidly has science progressed, that already the subject of life in other worlds has assumed a new aspect. Arguments which were hypothetical thirty years ago have either become certainties or been disproved. Doubtful points have been cleared up; a new meaning has been found even in those facts which were well known to both the disputants; and lastly, a new mode of research has been devised, which has not only revealed a number of surprising facts, but promises to work yet greater marvels in the years which are to come.

One is thus invited to discuss anew a subject which but a few years since seemed thoroughly sifted by the inquiries of the two eminent philosophers I have named. We stand in a position much more favorable for the formation of just views than that from which Whewell and Brewster surveyed the planetary and stellar systems. Never, since men first explored the celestial depths, has a series of more startling discoveries rewarded the labors of astronomers and physicists than during the past few years. Unhoped-for revelations have been made on every side. Analogies the most interesting have brought the distant orbs of heaven into close relationship with our own earth, or with the central luminary of the planetary scheme. And a lesson has been taught us which bears even more significantly on our views respecting the existence of other worlds: we have learned to recognize within the solar system, and within the wondrous galaxy of which our sun is a constituent orb, a variety of structure and a complexity of detail, of which but a few years ago astronomers had formed but the most inadequate conceptions.

My object, then, in the pages which follow, is not solely to establish the thesis that there are other worlds than ours, but to present, in a new and I hope interesting light, the marvellous discoveries which have rewarded recent scientific researches.

Judged merely according to their direct significance, these discoveries are well calculated to excite our admiration for the wonderful works of God in His universe, and for the far-reaching scope of the mental powers which He has given to His creature Man. But it is when we consider recent discoveries in their relation to the existence of other worlds, when we attempt to form a conception of the immense varieties of the forms of life corresponding to the innumerable varieties of cosmical structure disclosed by modern researches, that we recognize the full significance of those discoveries. Although the growth of our knowledge is ever accompanied by a proportional growth of our estimate of the unknown, we seem already entitled to say that we have

> Come on that which is, and caught
> The deep pulsations of the world,
> Æonian music, measuring out
> The steps of time.

CHAPTER I.

WHAT OUR EARTH TEACHES US.

BEFORE proceeding to consider the various circumstances under which the worlds or systems which surround us appear to subsist, it may be well to inquire how far we have reason to conclude, from the consideration of our own earth and its inhabitants, that the Creator has designed the orbs which exist throughout space for the support of living creatures.

It would not be just to argue directly from the fact that the earth is inhabited to the conclusion that the other planets are inhabited also, nor thence to the conclusion that other stars have, like our sun, their attendant worlds, peopled with various forms of life. An analogy founded on a single instance has no logical force. And it is doubtful whether we have not, in the moon, an instance which would as effectually serve to support a directly opposite conclusion. It seems all but certain, as we shall presently have occasion to show, that no part of the moon's globe is inhabited by living creatures. Certainly she is inhabited by none which bear the least resemblance to those existing on our earth. Thus it might fairly be urged that, since one of the two orbs respecting which we know most appears to be uninhabited, there remains no probable argument in favor of the view that other orbs besides our earth are the abode of living creatures.

Yet the earth in reality supplies an argument of great force, when we consider the evidence she presents in another light. The mere fact that this world is inhabited is, as we have seen, little; but we shall find that the way in which life is distributed over the earth's surface is full of significance.

If we range over the earth, from the arctic regions to the torrid zone, we find that none of the peculiarities which mark the several regions of our globe suffice to banish life from its surface. In the bitter cold within the arctic circles, with their strange alternations of long summer days and long winter nights, their frozen seas, perennial ice, and scanty vegetation, life flourishes in a hundred various forms. On the other hand, the torrid zone, with its blazing heat, its long-continued droughts, its strange absence of true seasonal changes, and its trying alternations of oppressive calms and fiercely-raging hurricanes, nourishes even more numerous and more various forms of life than either of the great temperate zones. Around mountain-summits as in the depths of the most secluded valleys, in mid-ocean as in the arid desert, in the air as beneath the surface of the earth, we find a myriad forms of life.

But this is far from being all. Various as are the physical habitudes which we encounter as we travel over the surface of our globe, we are able to trace the existence of other varieties even more remarkable. The geologist has been able to turn back a few leaves of the earth's past history, and, though the pages have been defaced and mutilated by Time's unsparing hand, he is yet able to read in them of many strange vicissitudes to which the continents and oceans of our globe have been exposed. But, far back as he can trace the earth's history, and already he counts her age by millions of years,[*] he finds no evidence of an epoch when life was absent from her surface. Nay, if he reads aright the mysterious lesson which the blurred letters teach him, he is led to believe that, at the most distant epoch to which his researches have extended, there was the same wonderful variety in the forms of life as at the present day. He can, indeed, find the scattered remains of only a few of those old-world creatures; but he recognizes, in those which have been preserved, the clearest evidence that thousands of others must have existed around them. He knows that, of a million creatures now existing, scarcely one will leave to future ages any record of its existence; he sees whole races vanishing from the earth, leaving no trace behind them; and he is thus able to form an estimate of the enormous extent by which the creatures now existing, scarcely one will leave to future ages outnumbered those whose scattered remains attest their former existence upon the earth.

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[*] The results of the recent deep-sea dredging expeditions, though they have an obvious bearing on the question of the relative ages of the various strata of our earth, do not appreciably affect our estimate of the range of time during which this world has been the abode of living creatures. We can no longer assume that adjacent rocks which differ in character are necessarily different in age: but we have enough evidence, from superimposed strata, to prove the enormous antiquity of the earlier formations. The researches of Dr. Carpenter and his fellow-workers have a most important bearing, however, on the subject of the present chapter, and supply a more forceful analogy, perhaps, than any dwelt on in the text, in favor of the view that, under the widest varieties of condition, Nature may be most prodigal of life.

Here, then, we have analogies which there is no mistaking. We see that not only is Nature careful to fill all available space with living forms, but that no time over which our researches extend has found her less prodigal of life. We see that, within very wide limits, she has a singular power of adapting living creatures to the circumstances which surround them. Nor is this lesson affected — like the general lesson drawn from the mere fact of the earth's being inhabited — by any thing we can learn from the aspect of our satellite. For the arguments against the presence of living creatures on the moon are founded on the evidence we have that the physical habitudes of that orb are outside the limits — wide as they seem to be — within which Nature can effect the adaptation we have spoken of.

In fact, if we consider rightly, the argument which has been drawn from the moon's presumed unfitness to be the abode of living creatures is so founded on terrestrial analogies as to leave the contrary argument unaffected. We have to assume that the argument drawn from the analogy of the earth is forceful before we can form any opinion at all respecting the moon's habitability. And, in any case, no argument can be drawn from the moon's unfitness for the support of life, against the view that, where orbs fit for the support of life exist, there Nature has provided such classes of living creatures as are adapted to the special habitudes of those orbs.

The moon teaches us, however, that the Creator has not intended all the celestial bodies to be at all times habitable. The sun also teaches the same lesson. And it is necessary that we should consider how far the evidence presented by our own earth may serve to elucidate this teaching. We shall see, as we proceed, that terrestrial analogies afford a very sure guide in the midst of many perplexities which the study of the worlds around us presents to our contemplation.

Let us trace out the various degrees of fitness or unfitness for the support of particular forms of life, which we recognize in various regions of our earth.

Often, where there exists so slight a difference between two regions of the earth that, to ordinary observation, it would appear that the forms of life existing in one should be well adapted to the other also, we yet find that this is not the case. Some minute peculiarity of soil, or climate, or vegetation, will render one region absolutely uninhabitable by a race which lives and thrives in the other. Darwin mentions several instances in which an apparently insignificant change in the circumstances under which a particular race has thriven, and sometimes a change which does not, at first sight, appear to be in the least connected with the well-being of the race, has led to its gradual disappearance. And it seems demonstrated that even the slow processes of change to which every part of the earth is subjected would suffice to destroy a number of the races now subsisting on its surface, were the characteristics of those races unalterable. But, as the physical habitudes of their abode slowly change, the various races of living creatures slowly change also, so as to adapt themselves continually to the varying circumstances under which they live.

The lesson taught us by this peculiarity is very obvious. On the one hand, we see that it would be by no means sufficient to indicate a general resemblance between the physical habitudes of our earth and those of some far-distant planet, in order to prove that that planet is the abode of living creatures resembling those on our own earth. But, on the other hand, we are taught that the existence of differences sufficient to render a distant planet an unsuitable abode for such creatures as we are familiar with, cannot force upon us the conclusion that the planet is uninhabited. On the contrary, the circumstance we have been considering teaches us, that such differences as would suffice to banish life of certain kinds are insufficient to banish life of all kinds, or even to render less abundant the forms of life which exist under those changed conditions.

And now we may proceed a step farther. On our earth we find differences of climate and of physical habitudes generally, which are much more important than those hitherto dealt with. We see that not only would certain races perish in the long-run, if removed from their own abode to other parts of the earth, but that, in some instances, the process of destruction would be very rapid indeed. If we were to remove the polar bears from their arctic fastnesses to tropical, or even to the warmer parts of temperate regions, a very few years would see the end of the whole race. The races inhabiting steppes and prairies would quickly perish, if removed to mountain-regions. Those accustomed to a moisture-laden air and abundant vegetation would not survive long if removed to the desert.

In some races, indeed, we find a power of enduring such changes which very far exceeds that possessed by other races. Those creatures, for example, which man has domesticated seem capable of enduring a variety of climate or of circumstances, which would destroy the seemingly more vigorous races which have not been subdued to the yoke of man.[*]

Even man himself, however, though he possesses in an unrivalled degree the power of enduring in safety the most complete change of climate, scene, and circumstances, is yet limited, in a certain sense, in his power of migration. The Englishman, for example, can endure the fiercest heat of the tropics or the bitterest cold of arctic and antarctic regions. But he cannot safely attempt to found true colonies in every part of the earth's surface. Our countrymen in India must send their children to be reared in England, if they wish them to grow up strong and vigorous. There can be little doubt that if a thousand men and women from this country were to settle in certain parts of India (not at any time intermarrying with the natives), the colony would have disappeared within a couple of centuries.

Here we have a second degree of unfitness, according to which certain countries would quickly become depopulated, if supplied with inhabitants from certain other countries. We are taught the same lesson as before, but in a more striking manner. We see that differences exist within the confines of our own earth, which render particular countries absolutely uninhabitable by particular races, insomuch that, though the individual might survive, the race itself would quickly perish. And we see, on the other hand, that these countries are not uninhabited, or even less fully peopled with living creatures, than seemingly more fortunate abodes.

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[*] Humboldt tells us that "the pliability of the organization of those animals which man has subjected to his sway, enables horses, cows, and other species ot European origin, to lead for a time an amphibious life, surrounded by crocodiles, water-serpents, and manatees. When the rivers return again to their beds, the horses roam in the savannah, which is then spread over with a fine odoriferous grass; and enjoy, as in their native climate, the renewed vegetation of spring."

Now, if some impassable barrier prevented the inhabitants of one country from visiting others, while yet it was possible to learn something of the conditions prevailing in other regions, how readily the conclusion might be reached, that some at least of those inaccessible regions must be wholly uninhabited, simply because their physical habitudes appeared unsuited to the wants of the only creatures with which the observer was familiar. Who would believe, for example, that men can live, and not only live but thrive and multiply, in the frost-bound regions within the Arctic circle, if travellers had not visited the Esquimaux races, and witnessed the conditions under which they subsist? Again, if we knew nothing of India, and some one pictured to us the intense heat of the Indian sun, the strange alternations of weather which replace to the Indian the seasonal changes we are familiar with, and all the other circumstances which render tropical regions so different from our English home, who could believe that, amid those seemingly unendurable vicissitudes, there are races of men that thrive and multiply, even as our people in their temperate zone? *

Therefore, in examining the circumstances of other worlds than ours, it will not be sufficient to prove that certain orbs would obviously not be habitable by the races subsisting on the earth, in order to enforce the conclusion that no living creatures subsist at all upon their surface.

Yet another step farther, however. There are regions of the earth where the *individuals* of races belonging to other regions quickly perish. The air of our own England is death to many creatures. And, indeed, there is not a spot in the whole world which would not be fatal in a brief space to many animals and plants belonging to other regions. Yet each spot, though thus fatal to certain races, is inhabited by numbers of others, which live and thrive upon its surface.

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* Perhaps the most striking instance of man's power of living under circumstances seemingly the most unfavorable, is to be found in the fact that, though the strongest traveller is affected seriously by the rarity of the air at great elevations, yet races of men live and thrive in Potosi, Bogota, and Quito, and — to use the words of a modern writer — that bull-fights should be possible at an elevation at which Saussure hardly had energy to consult his instruments, and where even his guides fainted as they tried to dig a small hole in the snow.

Here, then, is our third lesson. We are taught, by the analogy of our earth, that it is not even sufficient to show that a planet would be an abode quickly fatal to all the living creatures subsisting on our globe, to prove that it is therefore uninhabited.

But we have yet a stronger argument to touch on. There are regions of our earth to which creatures from other regions cannot be removed without being immediately killed. The warm-blooded animal perishes, if placed for a brief space under water. The fish perishes, if placed for a brief space on the earth.* What could be more wonderful to us, were we not familiar with the fact, than that there are living creatures within the depths of that ocean, beneath whose surface we ourselves, and the land creatures we are familiar with, cannot remain alive many minutes? If fishes could reason, how could they believe that creatures can live in comfort in that element which is death to them? Yet land and river and sea are alike peopled with living creatures, each race as well adapted as its fellows to the circumstances in which it is placed.

We are taught, then, yet another lesson. We see that, even though we could prove that every living creature on this earth would at once perish if removed to another orb, yet we cannot thence conclude that that orb is uninhabited. On the contrary, the lesson conveyed by our earth's analogy leads to the conclusion that many worlds may exist, abundantly supplied with living creatures of many different species, where yet every form of life upon our earth — bird, beast, or fish, reptile, insect, or animalcule — would perish in a few moments.†

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* Perhaps the fact that there are certain kinds of fish which cannot only live out of water, but can travel across the dry land, or climb trees, affords an even more striking instance of Nature's power of adapting creatures to the circumstances which surround them.

† I might add to the instances here cited, many others which seem even more striking. I have already referred to Dr. Carpenter's discovery, that in the depths of the Atlantic, where the pressure of the sea is so enormous that no ordinary instruments can resist its effects, where it had even been thought that no light can penetrate, there are myriads of living creatures having even organs of vision. We know, too, that in strong acids which would instantly kill bird, beast, fish or insect placed within them, there exist and thrive minute creatures, adapted by Nature to the strange conditions in which they are placed. Even in the bowels of the earth and in the very neighborhood of active volcanoes, we find the volcano-fish existing in such countless thousands, that, when they are from time to time vomited forth by the erupting

There remains yet a last lesson to be drawn from terrestrial analogies. On the earth there are regions where no form of life exists or can exist. Within the flaming crater of the volcano, or in the frozen heart of the iceberg, no living creature has its being. Yet even here Nature proves to us that the great end and aim of all her working is to afford scope and room for new forms of life, or to supply the wants of those which already exist. The volcano will die out, and the scene of its activity will one day become the abode of myriads of living creatures who would have perished in a moment in its consuming fires. The iceberg will melt, and its substance will once again be peopled with busy life. But this is little. It is the work of which volcano and iceberg are the signs, which most significantly teaches us what is Nature's real aim. The volcano is the index of those busy subterranean forces which are remodelling the earth's frame, slowly changing the level of the land, making continents of oceans and oceans of continents, preserving and vivifying all things, while all things seem to suffer a gradual destruction. The iceberg, too, has its work in remodelling and fashioning the surface of new continents. But it exhibits also the action of Nature for the present benefit of the creatures which exist upon the earth. It acts an important part in the formation and maintenance of the system of oceanic circulation on which the welfare of land creatures and water creatures so largely depends. And so of a multitude of other phenomena, which appear at first sight significant rather of the destructive than of the life-preserving character of Nature. The tornado and the thunder-storm, the earthquake and the volcano — nay, even the dreaded returns of plague and pestilence, have each a more powerful influence by far toward the preservation than they have toward the destruction of life.

We see, then, that even when we can prove that an orb in space is so circumstanced that no life could by any possibility exist upon its surface; if it were the scene of a fierce and destructive turmoil, one moment of which would suffice to destroy every living creature now existing upon the earth; if its whole mass were heated to a degree a thousandfold more intense than

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mountain, their bodies are strewn over enormous regions, and, as they putrefy beneath the sun's rays, spread pestilence and disease among the inhabitants of the neighboring districts.

that of the fiercest heat we know of; if its surface were bound in a cold compared with which our arctic frosts would seem like tropical heat; or even if the most rapid alternation of these extremes took place upon and within it; even then we could not conclude that the principal purpose for which the Almighty had created it had not been the support of life, either in long-past ages, or in ages yet to come. And lastly, though we could safely assert of any celestial object, that neither now, nor at any past or future time, could it serve as the abode of living creatures, yet we are led by terrestrial analogies to the conclusion that it has yet been created to support life in other ways. So that those very orbs, of which it seems safest to assert that they are, have ever been, and must ever remain uninhabited, speak to us, no less strongly than those which appear best suited for habitation, of the existence of other worlds than ours.

CHAPTER II.

WHAT WE LEARN FROM THE SUN.

I DO NOT propose to dwell in this chapter on the views which have been propounded respecting the sun's habitability. It is not merely that I regard those views as too *bizarre* and fanciful to find place in a serious consideration of the subject I am dealing with, nor that the progress of recent observation has rendered them utterly untenable, but that, in fact, they do not belong to what the sun teaches us. I wish to consider only the real evidence which the sun affords respecting the scheme of creation, to dwell upon the purposes which he subserves in the economy of the solar system, and thence to deduce a lesson respecting those other suns scattered throughout space, which we call the fixed stars.

Let us first endeavor to form adequate conceptions respecting the dimensions of the great central luminary of the solar system.

Let the reader consider a terrestrial globe three inches in diameter, and search out on that globe the tiny triangular speck which represents Great Britain. Then let him endeavor to picture the town in which he lives as represented by the minutest pin-mark that could possibly be made upon this speck. He will then have formed some conception, though but an inadequate one, of the enormous dimensions of the earth's globe, compared with the scene in which his daily life is cast. Now, on the same scale, the sun would be represented by a globe about twice the height of an ordinary sitting-room. A room about twenty-six feet in length, and height, and breadth, would be required to contain the representation of the sun's globe on this scale, while the globe representing the earth could be placed in a moderately large goblet.

Such is the body which sways the motions of the solar system. The largest of his family, the giant Jupiter, though of dimensions which dwarf those of the earth or Venus almost to nothingness,

JUPITER

as seen on Jany 30th 1870 9.30 P.M. by J. Browning F.R.A.S.

with a 12 inch Silver on Glass Reflector

would yet only be represented by a thirty-two inch globe, on the scale which gives to the sun the enormous volume I have spoken of. Saturn would have a diameter of about twenty-eight inches, his ring measuring about five feet in its extreme span. Uranus and Neptune would be little more than a foot in diameter, and all the minor planets would be less than the three-inch earth. It will thus be seen that the sun is a worthy centre of the great scheme he sways, even when we merely regard his dimensions.

The sun outweighs fully seven hundred and forty times the combined mass of all the planets which circle around him, so that, when we regard the energy of his attraction, we still find him a worthy ruler of the planetary scheme.

But, after all, the enormous volume and mass of the sun form the least important of his characteristics as the ruling body of the solar system. It is when we contemplate him as the source whence the supplies of heat and light required by our own world and the other planets are plentifully bestowed, that we see what is his chief office in the economy of the planetary scheme.

Properly speaking, the physical constitution of the sun only requires to be dealt with in such a work as the present in so far as it is directly associated with the sun's action upon the worlds around him, or as it may bear on the question of the constitution of those worlds. But the subject is so interesting, and it would indeed be so difficult to draw a line of demarcation between the facts which bear upon the question of other worlds and those which do not, that I may be permitted to enter at some length into a consideration of the solar orb, as modern physical discoveries present it to our contemplation.

The study of solar physics may be said to have commenced with the discovery of the sun-spots, about two hundred and sixty years ago. These spots were presently found to traverse the solar disk in such a way as to indicate that the sun turns upon an axis once in about twenty-six days. Nor will this rotation appear slow, when we remember that it implies a motion of the equatorial parts of the sun's surface at a rate exceeding some seventy times the motion of our swiftest express trains.

Next came the discovery that the solar spots are not surface-stains, but deep cavities in the solar substance. The changes of appearance presented by the spots as they traverse the solar disk led Dr. Wilson to form this theory so far back as 1779; but, strangely enough, it is only in comparatively recent times that the hypothesis has been finally established, since even within the last ten years a theory was put forward which accounted satisfactorily for most of the changes of appearance observed in the spots, by supposing them to be due to solar clouds hanging suspended at a considerable elevation above the true photosphere.

Sir William Herschel, reasoning from terrestrial analogies, was led to look on the spot-cavities as apertures through a double layer of clouds. He argued that, were the solar photosphere of any other nature, it would be past comprehension that vast openings should form in it, to remain open for months before they close up again. Whether we consider the enormous rapidity with which the spots form and with which their figure changes, or the length of time that many of them remain visible, we find ourselves alike perplexed, unless we assume that the solar photosphere resembles a bed of clouds. Through a stratum of terrestrial clouds openings may be formed by atmospheric disturbances, but while undisturbed the clouds will retain any form once impressed upon them, for a length of time corresponding to the weeks and months during which the solar spots endure.

And because the solar spots present two distinct varieties of light, the faint penumbra and the dark umbra or nucleus, Herschel saw the necessity of assuming that there are two beds of clouds, the outer self-luminous and constituting the true solar photosphere, the inner reflecting the light received from the outer layer, and so shielding the real surface of the sun from the intense light and heat which it would otherwise receive.

But while recent discoveries have confirmed Sir William Herschel's theory about the solar cloud-envelopes, they have by no means given countenance to his view that the body of the sun may possibly be cool. The darkness of the nucleus of a spot is found, on the contrary, to give proof that in that neighborhood the sun is hotter, because it parts less readily with its heat. We shall see presently how this is. Meantime let it be noticed, in passing, that a close scrutiny of large solar spots has revealed the existence of an intensely black spot in the midst of the umbra. This black spot must be regarded as the true nucleus.

The circumstance that the spots appear only on two bands of the sun's globe, corresponding to the subtropical zones on our own earth, led the younger Herschel to conclusions as important as those which his father had formed. He reasoned, like his father, from terrestrial analogies. On our own earth the subtropical zones are the regions where the great cyclonic storms have their birth, and rage with their chief fury. Here, therefore, we have the analogue of the solar spots, if only we can show reason for believing that any causes resembling those which generate the terrestrial cyclone operate upon those regions of the sun where the solar spots make their appearance.

We know that the cyclone is due to the excess of heat at the earth's equator. It is true that this excess of heat is always in operation, whereas cyclones are not perpetually raging in subtropical climates. Ordinarily, therefore, the excess of heat does not cause tornadoes. Certain aërial currents are generated, whose uniform motion suffices, as a rule, to adjust the conditions which the excess of heat at the equator would otherwise tend to disturb. But when through any cause the uniform action of the aërial currents is either interfered with, or is insufficient to maintain equilibrium, then cyclonic or whirling motions are generated in the disturbed atmosphere, and propagated over a wide area of the earth's surface.

Now we recognize the reason of the excess of heat at the earth's equator, in the fact that the sun shines more directly upon that part of the earth than on the zones which lie in higher latitudes. Can we find any reason for suspecting that the sun, which is not heated from without as the earth is, should exhibit a similar peculiarity? Sir John Herschel considers that we can. If the sun has an atmosphere extending to a considerable distance from his surface, then there can be little doubt that, owing to his rotation upon his axis, this atmosphere would assume the figure of an oblate spheroid, and would be deepest over the solar equator. Here, then, more of the sun's heat would be retained than at the poles, where the atmosphere is shallowest. Thus, that excess of heat at the solar equator which is necessary to complete the analogy between the sun-spots and terrestrial cyclones, seems satisfactorily established.

It must be remarked, however, that this reasoning, so far as the excess of heat at the sun's equator is concerned, only removes the difficulty a step. If there were indeed an increased depth of atmosphere over the sun's equator sufficing to retain the requisite excess of heat, then the amount of heat we receive from the sun's equatorial regions ought to be appreciably less than the amount emitted from the remaining portions of the solar surface. This is not found to be the case, so that, either there is no such excess of absorption, or else the solar equator gives out more heat, in other words, is essentially hotter, than the rest of the sun. But this is just the peculiarity of which we want the interpretation.

It may be taken for granted, however, that there is an analogy between the sun-spots and terrestrial cyclonic storms, though as yet we are not very well able to understand its nature.

Then next we come to one of the most interesting discoveries ever made respecting the sun — the discovery that the spots increase and diminish in frequency in a periodic manner. We owe this discovery to the laborious and systematic observations made by Herr Schwabe, of Dessau. In these pages any account of his work would be out of place. We need only dwell upon the result, and upon other discoveries which have been made by observers who have taken up the same work.

Schwabe found in the course of about ten and a half years, the solar spots pass through a complete cycle of changes. They become gradually more and more numerous up to a certain maximum, and then as gradually diminish. At length the sun's face becomes not only clear of spots, but a certain well-marked darkening around the border of his disk disappears altogether for a brief season. At this time the sun presents a perfectly-uniform disk. Then gradually the spots return, become more and more numerous, and so the cycle of changes is run through again.

The astronomers who have watched the sun from the Kew Observatory have found that the process of change by which the spots sweep in a sort of "wave of increase" over the solar disk is marked by several minor variations. As the surface of a great sea-wave will be traversed by small ripples, so the gradual increase and diminution in the number of the solar spots are characterized by minor gradations of change, which are sufficiently well marked to be distinctly cognizable.

There seems every reason for believing that the periodic changes thus noticed are due to the influence of the planets upon the solar photosphere, though in what way that influence is exerted is not at present perfectly clear. Some have thought that the mere attraction of the planets tends to produce tides of some sort in the solar envelopes. Then, since the height of a tide so produced varies as the cube or third power of the distance, it has been thought that a planet when in perihelion would generate a much larger solar tide than when in aphelion. So that, as Jupiter has a period nearly equal to the sun-spot period, it has been supposed that the attractions of this planet are sufficient to account for the great spot-period. Venus, Mercury, the Earth, and Saturn have, in a similar manner, been rendered accountable for the shorter and less distinctly marked periods.

Without denying that the planets may be, and probably are, the bodies to whose influence the solar-spot periods are to be ascribed, I yet venture to express very strong doubts whether the attraction of Jupiter is so much greater in perihelion than in aphelion as to account for the fact that whereas at one season the face of the sun shows many spots, at another it is wholly free from them.*

However, we are not at present concerned so much with the explanation of facts as with the facts themselves. We have to consider rather what the sun is and what he does for the solar system, than why these things are so.

Let us note, before passing to other circumstances of interest connected with the sun, that the variable condition of his photosphere must cause him to change in brilliancy as seen from vast distances. If Herr Schwabe, for instance, instead of observing the sun's spots from his watch-tower at Dessau, could have removed himself to a distance so enormous that the sun's disk would have been reduced, even in the most powerful telescope, to a mere point of light, there can be no doubt that the only effect which he would have been able to perceive would have been a gradual increase and diminution of brightness, having a period of about ten and a half years.

Our sun, therefore, viewed from the neighborhood of any of the stars, whence undoubtedly he would simply appear as one among many fixed stars, would be a " variable," having a period of ten and a half years. And further, if an observer, viewing the sun from so enormous a distance, had the means of very accurately measuring its light, he would undoubtedly discover that, while the chief variation of the sun takes place in a period of ten and a half years, its light is subjected to minor variations, having shorter periods.

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* Recently Prof. Kirkwood has published a most interesting series of inquiries, going far to prove that the real secret of the planetary influences lies in the fact that the sun's surface is not uniform, and that on a certain solar longitude the planetary influences are more effective than elsewhere.

The discovery that the periodic changes of the sun's appearance are associated with the periodic changes in the character of the earth's magnetism is the next that we have to consider.

It had long been noticed that, during the course of a single day, the magnetic needle exhibits a minute change of direction, taking place in an oscillatory manner. And, when the character of this vibration came to be carefully examined, it was found to correspond to a sort of effort on the needle's part to turn toward the sun. For example, when the sun is on the magnetic meridian, the needle has its mean position. This happens twice in the day, once when the sun is above the horizon, and once when he is below it. Again, when the sun is midway between these two positions — which also happens twice in the day — the needle has its mean position, because the northern and the southern ends make equal efforts (so to speak) to direct themselves toward the sun. Four times in the day, then, the needle has its mean position, or is directed toward the magnetic meridian. But, when the sun is not in one of the four positions considered, that end of the needle which is nearest to him is slightly turned away from its mean position, toward him. The change of position is very minute, and only the exact modes of observation made use of in the present age would have sufficed to reveal it. There it is, however, and this minute and seemingly unimportant peculiarity has been found to be full of meaning.

Had science merely measured this minute variation, the work would have given striking evidence of the exact spirit in which men of our day deal with natural phenomena. But science was to do much more. The variations of this minute variation were to be inquired into; their period was to be searched for; the laws by which they were regulated and by which their period might perhaps itself be rendered variable, were to be examined; and, finally, their relation to other natural laws was to be sought after. That Science should set herself to an inquiry so delicate and so difficult, in a spirit so exacting, was nothing unusual. It is thus that all the great discoveries of our day have been effected. But it is well that the reader should recognize the careful scrutiny to which natural phenomena have been subjected before the great laws we have to consider were made known. It is thought by many, who have not been at the pains to examine what Science is really doing in our day, that the wonders she presents to men's contemplation, the startling revelations which are being made from day to day, are merely dreams and fancies, which replace indeed the dreams and fancies of old times, but have no worthier claims on our belief. Those who carefully examine the history of science will be forced to adopt a very different opinion.

The minute vibrations of the magnetic needle, thus carefully watched — day after day, month after month, year after year — were found to exhibit a yet more minute oscillatory change. They waxed and waned within narrow limits of variation, but yet in a manner there was no mistaking. The period of this oscillatory change was not to be determined, however, by the observations of a few years.* Between the time when the diurnal vibration was least until it had reached its greatest extent, and thence returned to its first value, no less than ten and a half years elapsed, and a much longer time passed before the periodic character of the change was satisfactorily determined.

The reader will at once see what these observations tend to. The sun-spots vary in frequency within a period of ten and a half years, and the magnetic diurnal vibrations vary within a period of the same duration. It might seem fanciful to associate the two periodic series of changes together, and doubtless when the idea occurred to Lamont, it was an exceedingly bold expectation of finding it confirmed, that he examined the evidence bearing on the point. Judging from known facts, we may see reasons for such an expectation in the correspondence of the needle's diurnal vibration with the sun's apparent motion, and the law which has been found to associate the annual variations of the magnet's power with the sun's distance. But undoubtedly when the idea occurred to Lamont, it was an exceedingly bold one, and the ridicule with which the first announcement of the supposed law was received, even in scientific circles, suffices to show how unexpected that relation was, which is now so thoroughly established. For a careful comparison between the two periods has demonstrated that they agree most perfectly, not merely in length, but maximum for maximum, and minimum for minimum. When the sun-spots are most numerous, then the daily vibration of the magnet is most extensive, while, when the sun's face is clear of spots, the needle vibrates over its smallest diurnal arc.

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* The reader must not understand that the account here given presents in any sense even a general view of the labors of those who have studied the earth's magnetism. I touch only on those points by which the association between the earth's magnetism and the physical condition of the sun is most clearly indicated; because these points alone bear on the subject of this chapter. How they do so will appear further on.

Then the intensity of the magnetic action has been found to depend upon solar influences. The vibrations by which the needle indicates the progress of those strange disturbances of the terrestrial magnetism which are known as magnetic storms, have been found not merely to be most frequent when the sun's face is most spotted, but to occur simultaneously with the appearance of signs of disturbance in the solar photosphere. For instance, during the autumn of 1859, the eminent solar observer, Carrington, noticed the apparition of a bright spot upon the sun's surface. The light of this spot was so intense, that he imagined the dark glass which protected his eye had been broken. By a fortunate coincidence, another observer, Mr. Hodgson, happened to be watching the sun at the same instant, and witnessed the same remarkable appearance. Now it was found that the self-registering magnetic instruments of the Kew Observatory had been sharply disturbed at the instant when the bright spot was seen. And afterward it was learned that the phenomena which indicate the progress of a magnetic storm had been observed in many places. Telegraphic communication was interrupted, and, in some cases, telegraphic offices were set on fire; auroras appeared both in the northern and southern hemisphere during the night which followed; and the whole frame of the earth seemed to thrill responsively to the disturbance which had affected the great central luminary of the solar system.

The reader will now see why I have discussed relations which hitherto he may perhaps have thought very little connected with my subject. He sees that there is a bond of sympathy between our earth and the sun; that no disturbance can affect the solar photosphere, without affecting our earth to a greater or less degree. But if our earth, then also the other planets, Mercury and Venus, so much nearer the sun than we are, surely respond even more swiftly and more distinctly to the solar magnetic influences. But beyond our earth, and beyond the orbit of moonless Mars, the magnetic impulses speed with the velocity of light. The vast globe of Jupiter is thrilled from pole to pole as the magnetic wave rolls in upon it; then Saturn feels the shock, and then the vast distances beyond which lie Uranus and Neptune are swept by the ever-lessening yet ever-widening disturbance-wave. Who shall say what outer planets it then seeks? or who, looking back upon the course over which it has travelled, shall say that planets alone have felt its effects? Meteoric and cometic systems have been visited by the great magnetic wave, and upon the dispersed members of the one and the subtle structure of the other effects even more important may have been produced than those striking phenomena which characterize the progress of the terrestrial or planetary magnetic storms.

When we remember that what is true of a relatively great solar disturbance, such as the one witnessed by Messrs. Carrington and Hodgson, is true also (however different in degree) of the magnetic influences which the sun is at every instant exerting, we see that a new and most important bond of union exists between the members of the solar family. The sun not only sways them by the vast attraction of his gravity, not only illumines them, not only warms them, but he pours forth on all his subtle yet powerful magnetic influences. A new analogy between the members of the solar system is thus introduced, to reënforce those other analogies which have been held so strikingly to indicate that the ends for which our earth has been created are not different from those which the Creator had in view when He planned the other members of the solar system.*

And now we pass on to other discoveries, bearing at once and with equal force upon the relations between the various members of the solar scheme and upon the position which that scheme occupies in the universe.

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* I must remark here, once for all, that in speaking of the plans of the Creator, of His mode of working, or of the laws which He has established, I by no means intend such words to be taken literally. For want of better, such words as these must be employed in speaking of the relations between Almighty God and His universe. But in truth these relations are as inconceivable by us as infinity of space or infinity of time. We know that they exist, as certainly as we know that space and time are both infinite, but human language can no more indicate their nature than it can present to the mind an adequate picture of space or time.

Hitherto we have been considering the teachings of the telescope; we have now to consider what we have learned by means of an instrument of yet higher powers. As I shall have to refer very frequently, throughout this volume, to the teachings of the spectroscope, it will be well that I should briefly describe what it is that this instrument really effects. Were I simply to state the results of its use, without describing its real character, many of my readers would be disposed to believe that astronomers are as credulous as in reality they are exacting and scrupulous, where new facts and observations are in question.

The real end and aim of the telescope, as applied by the astronomer to the examination of the celestial objects, is to gather together the light which streams from each luminous point throughout space. We may regard the space which surrounds us on every side as an ocean without bounds or limits, an ocean across which there are ever sweeping waves of light, either emitted directly from the various bodies subsisting throughout space, or else reflected from their surfaces. Other forms of wave also speed across those limitless depths in all directions, but the light-waves are those which at present concern us. Our earth is as a minute island placed within the ocean of space, and to the shores of this tiny isle the light-waves bear their message from the orbs which lie like other isles amid the fathomless depths around us. With the telescope the astronomer gathers together portions of light-waves which else would have travelled in diverging directions. By thus intensifying their action, he enables the eye to become cognizant of their true nature. Precisely as the narrow channels around our shores cause the tidal wave, which sweeps across the open ocean in almost insensible undulations, to rise and fall through a wide range of variation, so the telescope renders sensible the existence of light-waves which would escape the notice of the unaided eye.

The telescope, then, is essentially *a light-gatherer.*

The spectroscope is used for another purpose. It might be called the *light-sifter.* It is applied by the astronomer to analyze the light which comes to him from beyond the ocean of space, and so to enable him to learn the character of the orbs from which that light proceeds.

The principle of the instrument is simple, though the appliances by which its full powers can alone be educed are somewhat complicated.

A ray of sunlight falling on a prism of glass or crystal does not emerge unchanged in character. Different portions of the ray are differently bent, so that when they emerge from the prism they no longer travel side by side as before. The violet part of the light is bent most, the red least; the various colors from violet through blue, green, and yellow, to red, being bent gradually less and less.

The prism then *sorts,* or *sifts,* the light-waves.

But we want the means of sifting the light-waves more thoroughly. The reader must bear with me while I describe, as exactly as possible in the brief space available to me, the way in which the first rough work of the prism has been modified into the delicate and significant work of the spectroscope. It is well worth while to form clear views on this point, because so many of the wonders of modern science are associated with spectroscopic analysis.

If, through a small round hole in a shutter, light is admitted into a darkened room, and a prism be placed with its refracting angle downward and horizontal, a vertical spectrum, having its violet end uppermost, will be formed on a screen suitably placed to receive it.

But now let us consider what this spectrum really is. If we take the light-waves corresponding to any particular color, we know, from optical considerations, that these waves emerge from the prism in a pencil exactly resembling in shape the pencil of white light which falls on the prism. They therefore form a small circular or oval image on their own proper part of the spectrum. Hence the spectrum is in reality formed of a multitude of overlapping images, varying in color from violet to red. It thus appears as a rainbow-tinted streak, presenting every gradation of color between the utmost limits of visibility at the violet and red extremities.

If we had a square aperture to admit the light, we should get a similar result. If the aperture were oblong, there would still be overlapping images; but if the length of the oblong were horizontal, then, since each image would also be a horizontally-placed oblong, the overlapping would be less than when the images were square. Suppose we diminish the overlapping as much as possible; in other words, suppose we make the oblong slit as narrow as possible? Then, unless there were in reality an infinite number of images distributed all along the spectrum from top to bottom, the images might be so narrowed as not to overlap; in which case, of course, there would be horizontal dark spaces or gaps in our spectrum. Or, again, if we failed in finding gaps of this sort by simply narrowing the aperture, we might lengthen the spectrum by increasing the refracting angle of the prism, or by using several prisms, and so on.

The first great discovery in solar physics, by means of the analysis of the prism (though the discovery had little meaning at the time), consisted in the recognition of the fact that, by means of such devices as the above, dark gaps or cross-lines *can* be seen in the solar spectrum. In other words, light-waves of the various gradations corresponding to all the tints of the spectrum from violet to red, do *not* travel to us from the great central luminary of our system. Remembering that the effect we call color is due to the length of the light-waves, the effect of red corresponding to light-waves of the greatest length, while the effect of violet corresponds to the shortest light-waves, we see that in effect the sun sends forth to the worlds which circle around him light-waves of many different lengths, but not of all. Of so complex and interesting a nature is ordinary daylight.

But spectroscopists sought to interpret these dark lines in the solar spectrum, and it was in carrying out this inquiry — which even to themselves seemed almost hopeless, and to many would appear an utter waste of time — that they lighted upon the noblest method of research yet revealed to man.

They examined the spectra of the light from incandescent substances (white-hot metals and the like), and found that in these spectra there are no dark lines.

They examined the spectra of the light from the stars, and found that these spectra are crossed by dark lines resembling those in the solar spectrum, but differently arranged.

They tried the spectra of glowing vapors, and they obtained a perplexing result. Instead of a number of dark lines across a rainbow-tinted streak, they found bright lines of various color.

Some gases would give a few such lines, others many, some only one or two.

Then they tried the spectrum of the electric spark, and they found here also a series of bright lines, but not always the same series. The spectrum varied according to the substances between which the spark was taken, and the medium through which it passed.

Lastly, they found that the light from an incandescent solid or liquid, when shining through various vapors, no longer gives a spectrum without dark lines, but that the dark lines which then appear vary in position, according to the nature of the vapor through which the light has passed.

Here were a number of strange facts, seemingly too discordant and too perplexing to admit of being interpreted. Yet one discovery only was wanting to bring them all into unison.

In 1859, Kirchhoff, while engaged in observing the solar spectrum, lighted on the discovery that a certain double dark line, which had already been found to correspond exactly in position with the double bright line forming the spectrum of the glowing vapor of sodium, was intensified, when the light of the sun was allowed to pass through that vapor. This at once suggested the idea, that the presence of this dark line (or rather, pair of dark lines) in the spectrum of the sun is due to the existence of the vapor of sodium in the solar atmosphere, and that this vapor has the power of absorbing the same order of light-waves as it emits. It would of course follow from this, that the other dark lines in the solar spectrum are due to the presence of other absorbent vapors in its atmosphere, and that the identity of these would admit of being established in the same way, supposing this general law to hold, that a vapor emits the same light-waves that it is capable of absorbing.

Kirchhoff was soon able to confirm his views by a variety of experiments. The general principles to which his researches led — in other words, the principles which form the basis of spectrum-analysis — are as follows:

1. An incandescent solid or liquid gives a continuous spectrum.
2. A glowing vapor gives a spectrum of white lines, each vapor having its own set of bright lines, so that, from the appearance of a bright-line spectrum, one can tell the nature of the vapor or vapors whose light forms the spectrum.

3. An incandescent solid or liquid shining through absorbent vapors gives a rainbow-tinted spectrum crossed by dark lines, these dark lines having the same position as the bright lines belonging to the spectra of the vapors; so that, from the arrangement of the dark lines in such a spectrum, one can tell the nature of the vapor or vapors which surround the source of light.*

The application of the new method of research to the study of the solar spectrum quickly led to a number of most interesting discoveries. It was found that, besides sodium, the sun's atmosphere contains the vapors of iron, calcium, magnesium, chromium, and other metals. The dark lines corresponding to these elements appear unmistakably in the solar spectrum. There are other metals, such as copper and zinc, which seem to exist in the sun, though some of the corresponding dark lines have not yet been recognized. As yet it has not been proved that gold, silver, mercury, tin, lead, arsenic, antimony, or aluminium, exist in the sun — though we can by no means conclude, nor indeed is it at all probable, that they are absent from his substance. The dark lines belonging to hydrogen are very well marked indeed in the solar spectrum, and, as we shall see presently, the study of these lines has afforded most interesting information respecting the physical constitution of the sun.

Now we notice at once how importantly these researches into the sun's structure bear upon the subject of this treatise. It would be indeed interesting to consider the actual condition of the central orb of the planetary scheme, to picture in imagination the metallic oceans which exist upon his surface, the continual evaporation from those oceans, the formation of metallic clouds, and the downpour of metallic showers upon the surface of the sun. But apart from such considerations, and viewing Kirchhoff's discoveries simply in their relation to the subject of other worlds, we have enough to occupy our attention.

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* To these may be added the following laws:

4. Light reflected from any opaque body gives the same spectrum as it would have given before reflection.

5. But if the opaque body be surrounded by vapors, the dark lines corresponding to these vapors make their appearance in the spectrum with a distinctness proportioned to the extent to which the light has penetrated those vapors before being reflected to us.

6. If the reflecting body be itself luminous, the spectrum belonging to it is superadded to the spectrum belonging to the reflected light.

7. Glowing vapors surrounding an incandescent source of light may cause bright lines or dark lines to appear in the spectrum, according as they are more or less heated; or they may emit just so much light as to make up for what they absorb, in which case there will remain no trace of their presence.

8. The electric spark presents a bright-line spectrum, compounded of the spectra belonging to the vapors of those substances between which, and of those through which, the discharge takes place. According to the nature of these vapors and of the discharge itself, the relative intensity of the component parts of the spectrum will be variable.

Lastly, the appearance of the spectrum belonging to any element will vary according to the circumstances of pressure and temperature under which the element may emit light.

If it could have been shown that, in all probability, the substance of the sun consists of materials wholly different from those which exist in this earth, the conclusion obviously to be drawn from such a discovery would be that the other planets also are differently constituted. We could not find any just reason for believing that in Jupiter or Mars there exist the elements with which we are acquainted, when we found that even the central orb of the planetary system exhibits no such feature of resemblance to the earth. But now that we know, quite certainly, that the familiar elements iron, sodium, and calcium, exist in the sun's substance, while we are led to believe with almost perfect assurance that all the elements we are acquainted with also exist there, we see at once that, in all probability, the other planets are constituted in the same way. There may of course be special differences: in one planet the proportionate distribution of the elements may differ, and even differ very markedly, from that which prevails in some other planet. But the general conclusion remains that the planets are formed of the elements which have so long been known as terrestrial; for we cannot recognize any reason for believing that our earth alone, of all the orbs which circle around the sun, resembles that great central orb in general constitution.*

Now, we have in this general law a means of passing beyond the bounds of the solar system, and forming no indistinct conceptions as to the existence and character of worlds circling around other suns. For it will be seen, in the chapter on the stars, that these orbs, like our sun, contain in their substance many of the so-called terrestrial elements, while it may not unsafely be asserted that all, or nearly all, those elements, and few or no elements unknown to us, exist in the substance of every single star that shines upon us from the celestial concave. Hence we conclude that around those suns also there circle orbs constituted like themselves, and therefore containing the elements with which we are familiar. And the mind is immediately led to speculate on the uses which those elements are intended to subserve. If iron, for example, is present in some noble orb circling around Sirius, we speculate not unreasonably respecting the existence on that orb — either now or in the past, or at some future time — of beings capable of applying that metal to the useful purposes which man makes it subserve. The imagination suggests immediately the existence of arts and sciences, trades and manufactures, on that distant world. We know how intimately the use of iron has been associated with the progress of human civilization, and though we must ever remain in ignorance of the actual condition of intelligent beings in other worlds, we are yet led, by the mere presence of an element which is so closely related to the wants of man, to believe, with a new confidence, that for such beings those worlds must in truth have been fashioned.

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* It will be seen, in the chapter on Meteors and Comets, that this conclusion has a most important bearing on the views we are to form respecting the original formation of the planetary scheme.

I would fain dwell longer on the thoughts suggested by the researches of Kirchhoff. Gladly too would I enter at length on an account of those interesting discoveries which have been made in connection with the last two total eclipses of the sun. The requirements of space, however, and some doubt as to the direct bearing of the last-named discoveries on the subject I have in hand, warn me to forbear. One point, however, remains, which is too intimately connected with my subject to be passed over.

I refer to the sun's corona.

It has been proved that the solar prominences consist of glowing vapors, hydrogen being their chief constituent. It has been found also, by comparing Mr. Lockyer's observations of the prominence-spectra with Dr. Frankland's elaborate researches into the peculiarities presented by the spectrum of hydrogen at different pressures, that even in the very neighborhood of the solar photosphere, these vapors probably exist at a pressure so moderate as to indicate that the limits of the sun's vaporous envelope cannot lie very far (relatively) from the outer solar cloud-layer.

Now, the solar corona has been seen, during total eclipses of the sun, to extend to a distance at least equal to the sun's diameter from the eclipsed orb. So that, assuming the corona to be a solar atmosphere, it would have a depth of about eight hundred and fifty thousand miles, and being also drawn toward the sun by his enormous attractive energy (exceeding more than twenty-seven times that of the earth), it could not fail to exert a pressure on his surface exceeding many thousand-fold that of our air upon the earth. In fact, such an atmosphere, let its outermost layers be as rare as we can conceive, would yet have its lower layers absolutely liquefied, if not solidified, by the enormous pressure to which they would be subjected. We cannot, then, believe this corona to be a solar atmosphere.

Yet it is quite impossible to dissociate the corona, either wholly or in part, from the sun. I am aware that physicists of eminence have attempted to do this, and not only so, but to make of the zodiacal light a terrestrial phenomenon. But they have overlooked considerations which oppose themselves irresistibly to such a conclusion.

In the first place, the mere fact that, during a total eclipse, the moon looks black, in the very heart of the corona, affords, when properly understood, the most conclusive evidence that the light of the corona comes from behind the moon. If the glare of our atmosphere could by any possibility account for the corona (which is not the case), then that glare should appear over the moon's disk also. That this is so is proved by the fact that, when the glare really does cover the moon, as while the sun is but slightly eclipsed, the moon is not projected as a black disk on the background of the *sky,* though, where her outline crosses the sun, it appears black, by contrast with the intensity of his light.* The point seems, however, too obvious to need discussion.

And, secondly, as Mr. Baxendell has pointed out, during totality the part of the earth's atmosphere between the eye and the corona is not illuminated by the sun. Over a wide space all round the sun we are looking through an atmosphere which is completely dark. In fact, if the earth's atmosphere alone were in question, we ought to see a dark or negative corona around the sun, the illuminated atmosphere only beginning to be faintly visible at a considerable angular distance from the sun. This argument, rightly understood, is altogether decisive of the question.*

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* It is also shown most conclusively, by a photograph of the eclipse of August, 1868, taken an instant before the totality. Here we see the glare trenching upon the moon's disk (elsewhere black), as it should theoretically. So soon as totality commenced, the glare had reached the moon's limb, whence it must immediately have passed quickly away.

But the spectroscope has given certain very perplexing evidence respecting the light of the corona, and it remains that we should endeavor to see how that evidence bears on the interesting problem which the corona presents to our consideration.

During the total eclipse of last August the American observers found that the spectrum of the corona is continuous, but crossed by certain bright lines. If we accept the absence of dark lines as established by the evidence (which is doubtful), this result seems at first sight very difficult to explain. Referring to the principles of spectroscopic analysis stated at pp. 27, 28, it will be seen that we should be led to infer that the corona consists of incandescent matter surrounded by certain glowing gases. It is difficult to suppose that this is the real explanation of the phenomenon.

Mr. Lockyer suggests that, if the corona shone by reflecting the solar light, the continuous spectrum might be accounted for by supposing the light from the glowing vapors around the sun to supply the part wanting where the solar dark lines are, and that some of these vapors shining yet more brightly would exhibit their bright lines upon the continuous background of the spectrum. This view, as applied by Mr. Lockyer to the theory that the corona is a terrestrial phenomenon, is untenable, for the reasons already adduced. But, independently of those reasons, there are others which render such a solution of the difficulty unavailable.

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* In fact, if we take the mode of reasoning by which Mr. Lockyer has endeavored to get over certain physical difficulties presently to be mentioned, we shall be able to point definitely to the place where his argument fails. He says, conceive a tiny moon placed so as to appear coincident with the centre of the sun's disk. There will be atmospheric glare as well as direct sunlight. Now, conceive this small moon to expand until it all but covers the sun. Still there will be glare and a certain small proportion of direct sunlight. So far his reasoning is most just. But when he allows his expanding moon to cover the sun, and to extend beyond the solar disk as in total eclipse, the atmospheric glare can no longer be assumed to exist all round the expanding moon: at the moment when the moon just hides the sun, the glare begins to leave the moon, a gradually-expanding black ring being formed round that body. It is only necessary to consider where the glare comes from to see that this must be so.

I have taken no account of diffraction here, because it has been abundantly proved that no corona of appreciable width could be formed around the moon during total eclipse by the diffraction of the rays of light as they pass near the moon's limb.

Now, remembering that we have two established facts for our guidance,—(i.) the fact that the corona cannot be a solar atmosphere, and (ii.) the fact that it must be a solar appendage — I think a way may be found toward a satisfactory explanation.

Let it be premised that the bright lines of the coronal spectrum correspond in position to those seen in the spectrum of the aurora, and that the same lines are seen in the spectrum of the zodiacal light, and in that of the phosphorescent light occasionally seen over the heavens at night.

Since we have every reason to believe that the light of the aurora is due to electrical discharges taking place in the upper regions of the air, we are invited to the belief that the coronal light may be due to similar discharges taking place between the particles (of whatever nature) constituting the corona.

Now, though the appearance of an aurora is due to some special terrestrial action (however excited), yet the material substances between which the discharges take place must be assumed to be at all times present in the upper regions of air. In all probability, they are the particles of those meteors which the earth is continually encountering. And since we know that meteor-systems must be aggregated in far greater numbers near the sun than near the earth, we may regard the coronal light as due to electrical discharges excited by the sun's action, and taking place between the members of such systems. Besides this light, however, there must necessarily be a large proportion of light reflected from these meteoric bodies. In this way the peculiar character of the coronal spectrum may be readily accounted for. We know, from the auroral spectrum, that the principal bright lines due to the electrical discharges would be precisely where we see bright lines in the coronal spectrum. But, besides these, there would be fainter bright lines corresponding to the various elements which exist in the meteoric masses. These elements, we know, are the same as those in the substance of the sun. Thus the bright lines would correspond in position with the dark lines of the solar spectrum. Hence, as light reflected by the meteors would give the ordinary solar spectrum, there would result from the combination a continuous spectrum, on which the bright lines first mentioned would be seen, as during the American eclipse.

What the polariscope has told us respecting the corona is in accordance with this view.

In the same way the quality of the zodiacal light admits of being perfectly accounted for, without resorting to the hypothesis that this phenomenon is a terrestrial one.*

The explanation thus put forward has at least the advantage of being founded on well-established relations. We know that the auroral light is associated with the earth's magnetism, and that meteoric bodies are continually falling upon the earth's atmosphere. We know, also, that the sun exerts magnetic influences a thousand-fold more intense than those of the earth, and that in his neighborhood there must be many million times more meteoric systems.

But we have other and independent reasons, which must not be overlooked, for considering the corona to be of some such nature as I have suggested. Leverrier has shown that there probably exists in the neighborhood of the sun a family of bodies whose united mass suffices appreciably to affect the motions of the planet Mercury. It would not be safe to neglect considerations thus vouched for.

Mr. Baxendell, also, has shown that certain periodic variations in the earth's magnetism point to the existence of such a family of bodies; and he has been able to assign to them a position according well with that determined by Leverrier.

Now, whatever opinion we form as to the exact character of the system of bodies pointed to by the researches of Leverrier and Baxendell — whether we suppose that system to form a zone around the sun,† or that (as I believe) the system is merely due to the aggregation of meteoric perihelia in the sun's neighborhood — we may be quite certain of this, that during a total solar eclipse the system could not fail to become visible. Hence there is a double objection to the view put forward by Mr. Lockyer and others. In the first place, it fails to account for the appearance presented by the corona; in the second place, it fails to render an account of the implied non-appearance of the system which, according to the researches of Leverrier and Baxendell, circles around the sun.

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* It was with some surprise that, at a late meeting of the Royal Astronomical Society, I heard Dr. Balfour Stewart put forward, even as a hypothesis, so startling a proposition as this. That the region of the counter-trades may be at times illuminated by electrical discharges will serve to account very well for the occasional phosphorescent appearance of the whole heavens at night — but the portion of the heavens illuminated by the zodiacal light has no relation whatever to the atmospheric region in which the counter-trades prevail. The hypothesis indeed, is wholly untenable.

† I am not here referring to Humboldt's notion that the zodiacal light is due to a zone of small bodies round the sun; a view which only derives importance from the fact that Sir John Herschel has been at the pains to contradict it. It need hardly be said, that Sir John Herschel's

It will be seen, in the chapter on "Meteors and Comets," how important a bearing these views respecting the nature of the corona have upon the history of the solar system. It has been partly for this reason that I have here briefly considered the matter; but there is another and a most important relation in which these views must be regarded.

We know that the sun is the sole source whence light and heat are plentifully supplied to the worlds which circle around him. The question immediately suggests itself — Whence does the sun derive those amazing stores of force from whence he is continually supplying his dependent worlds? We know that, were the sun a mass of burning matter, he would be consumed in a few thousand years. We know that, were he simply a heated body, radiating light and heat continually into space, he would in like manner have exhausted all his energies in a few thousand years — a mere day in the history of his system. Whence, then, comes the enormous supply of force which he has afforded for millions on millions of years, and which also our reason tells us he will continue to afford while the worlds which circle around him have need of it — in other words, for countless ages yet to come?

Now, there are two ways in which the solar energies might be maintained. The mere contraction of the solar substance, Helmholtz tells us, would suffice to supply such enormous quantities of heat, that if the heat actually given out by the sun were due to this cause alone, there would not, in many thousands of years, be any perceptible diminution of the sun's diameter. But, secondly, the continual downfall of meteors upon the sun would cause an emission of heat in quantities vast enough for

opinion has a weight which is altogether wanting to Humboldt's, so far as astronomical matters are concerned.

the wants of all the worlds circling round him; while his increase of mass from this cause would not be rendered perceptible in thousands of years, either by any change in his apparent size or by changes in the motions of his family of worlds.*

It seems far from unlikely that both these processes are in operation at the same time. Certainly the latter is, for we know, from the motions of the meteoric bodies which reach the earth, that myriads of these bodies must continually fall upon the sun. And if the corona and zodiacal light really be due to the existence of flights of meteoric systems circling around the sun, or to the existence in his neighborhood of the perihelia of many meteoric systems, then there must be a supply of light and heat from this source, very nearly if not quite sufficient to account for the whole solar emission.

It is well worthy of notice, too, that the association between meteors and comets has an important bearing on this question. We know that the most remarkable characteristic of comets is the enormous diffusion of their substance. Now, in this diffusion there resides an enormous fund of force. The contraction of a large comet to dimensions corresponding to a very moderate mean density would be accompanied by the emission of a vast supply of heat. And the question is worth inquiring into, whether we can indeed assume that the meteors which reach our atmosphere are solid bodies, and not rather of cometic diffusion; since it is difficult otherwise to account for the light and heat which they emit. Friction through the rarer upper strata of our atmosphere will certainly not account for these phenomena; nor, I think, will the compression of the atmosphere in front of the meteors; on the other hand, the sudden contraction of a diffused vapor would be accompanied by precisely such results. But, be this as it may, it is certain that a large portion of the substance of every comet is in a singularly diffused state. And since the meteoric systems circling in countless millions round the sun are, in all probability, associated in the most intimate manner with comets, we may recognize in this diffusion, as well as in the mere downfall of meteors, the source of an enormous supply of light and heat.

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* Altogether undue stress has been laid upon the probable change in the length of the year, owing to the downfall of meteors upon the sun's mass. It is forgotten that the crowded meteors forming the solar corona are *already* within the earth's orbit, and therefore already produce their full effect on the length of the year. The subsidence of all these bodies at once upon the sun would not affect the length of the year, though it would lead to certain modifications in the secular perturbations of the earth's orbit in figure and position.

And lastly, turning from our sun to the other suns which shine in uncounted myriads throughout space, we see the same processes at work upon them all. Each star-sun has its coronal and its zodiacal disks, formed by meteoric and cometic systems; for otherwise each would quickly cease to be a sun. Each star-sun emits, no doubt, the same magnetic influences which give to the zodiacal light and to the solar corona their peculiar characteristics. And thus the worlds which circle round those orbs may resemble our own in all those relations which we refer to terrestrial magnetism, as well as in the circumstance that on them also there must be, as on our own earth, a continual downfall of minute meteors. In those worlds, perchance, the magnetic compass directs the traveller over desert wastes or trackless oceans; in their skies, the aurora displays its brilliant streamers; while, amid the constellations which deck their heavens, meteors sweep suddenly into view, and comets extend their vast length athwart the celestial vault, a terror to millions, but a subject of study and research to the thoughtful.

CHAPTER III.

THE INFERIOR PLANETS.

IN considering the habitability of various portions of the solar system, we have to draw a marked distinction between the planets which travel within the orbit of the earth and those which lie beyond its range. So far, indeed, as our belief in these orbs being inhabited is concerned, we may apply the same processes of reasoning to one set of planets as to the other. Until it has been demonstrated that no form of life can exist upon a planet, the presumption must be that the planet is inhabited. But it is impossible to contemplate the various members of our solar system, without being led to consider their physical habitudes rather with relation to the wants of such creatures as exist upon our own earth, than merely with reference to the existence of life of some sort upon their surface. Viewing Venus and Mercury in this way, we have a different set of relations to deal with than we find among the outer planets. We are struck, at once, with the marked effects which seem associable with their comparative proximity to the sun's orb. This feature and the shortness of their period of revolution — that is, of their year — are the characteristic peculiarities we have to deal with.

I would willingly pay some attention here to the story of Vulcan, the planet which has been supposed to circle yet more closely than Mercury around the centre of our system, were it not for the great doubt in which the existence of this planet seems enshrouded. If, on the one hand, we have the evidence of Lescarbault that, on a certain day, and at a certain hour, he saw a dark object, round like a planet, crossing the face of the sun, we have also the evidence of Liais, whose name is much better known among astronomers, that at that very hour there was no such object on the solar disk. There is nothing to render the existence of an intra-Mercurial planet at all unlikely; and there are many observations which scarcely seem explicable on any other hypothesis. Still, as yet we have not that clear and unmistakable evidence which would permit me to speak of Vulcan as a planet known to astronomers, and I wish, while within the bounds of the solar system, to limit myself to the consideration of bodies which have been recognized and examined.

Mercury circles around the sun in the brief period of eighty-eight days, or rather less than three of our months. So that, if the planet has seasons, these must be severally about three weeks long. His distance from the sun varies between somewhat wide limits, owing to the eccentricity of his orbit. When he is nearest to the sun, he receives ten and a half times more light and heat from that luminary than we do; but, when he removes to his greatest distance, the light and heat he receives are reduced by more than one-half. Even then, however, the sun blazes in the skies of Mercury with a disk four and a half times larger than that which he presents to the observer on earth.

Undoubtedly these peculiarities, the shortness of the Mercurial year, and the immense amount of light and heat poured by the sun upon the planet, are circumstances which do not encourage, at first sight, the belief that any creatures can subsist upon this planet, resembling those with which we are familiar. We see, at once, that all forms of vegetation in Mercury must differ in a very striking manner from those which exist upon the earth, because their structure has to be adapted to much more rapid changes of temperature. And the existence of a totally distinct flora suggests at once the belief that animal life on Mercury must be very different from what we see around us.

Let us, however, proceed a few steps farther.

It has been found that Mercury rotates upon his axis, and if we may put faith in the observations of Schröter, the Mercurial day is only a few minutes longer than our own. But, though the fact of the planet's rotation has been observed, it has not been found possible to determine in what position the axis of rotation lies. It has been said that the planet's equator is much more inclined than the earth's to the plane in which the planet travels; but little reliance can be placed on the evidence which has been adduced in favor of this view.

We are thus left altogether in doubt as to the nature of the Mercurial seasons. That the planet has seasons of some sort we are certain, because, even if the axis were so placed that perpetual spring reigned upon the planet — I mean, that the days and nights were at all times and in all places equal — yet his varying distance from the sun would give changes of temperature quite as marked as those which characterize our seasons in England, and very much more marked than those known in tropical regions. Of course, if this is the actual arrangement, there are different climates in different parts of the planet. Near his poles, the sun, though visible for half the Mercurial day, attains yet but a low elevation above the horizon; just as he does on a spring day within our own polar circles. At the equator the sun passes day after day to the zenith, and pours down upon the planet an amount of light and heat far exceeding the light and heat of our tropical climates. A sun immediately overhead, and showing a diameter varying from more than twice to more than three times that of our sun, must be a noble and maybe a terrible phenomenon in the skies of Mercury.

There is yet another arrangement by which to a portion of the planet, at any rate, the Mercurial seasons might be tempered. If his axis is so placed that what would be the winter season, were his orbit not eccentric, takes place, for one hemisphere, when the planet is nearest to the sun, then undoubtedly it may very well happen (the inclination of his axis being suitably adjusted) that this so-called winter season is the warmest part of the year for that hemisphere. In this case, there would be the least possible violence in the succession of the Mercurial seasons for that hemisphere. But in the other hemisphere the seasonal changes would be correspondingly intensified.

In either of these cases, it is readily conceivable that even forms of life resembling those we are acquainted with on earth might exist on Mercury, and that without any special provision for tempering the great heat and light of the sun. Those regions which correspond to our temperate and tropical zones would indeed scarcely be habitable; but the polar regions of the planet would not form a disagreeable abode.

If, however, the equator of the planet is very much inclined to the plane in which Mercury travels, it cannot be doubted that no form of life known upon earth can possibly exist upon Mercury, *without* some special arrangements for tempering the seasonal changes. This will appear when we come to deal with the effect of the great inclination which some astronomers have ascribed to the equator of Venus, and therefore we need not consider the relation with regard to Mercury, of whose axial inclination no trustworthy information has hitherto been obtained.

It remains for us to consider what sort of provision may have been made to temper the great heat poured by the sun upon Mercury.

The climate of a planet, considered generally, is largely influenced by the nature of the planet's atmosphere. We have very clear evidence on this point, in the effects which we notice on our own earth. If we ascend to the summit of a lofty mountain, we find the air much colder than at its base. In India, though the full heat of a tropical sun is poured day after day upon the snowy summits of the Himalayas, yet the air continues colder than in the bitterest midwinter weather experienced by us in England. Not that the solar rays have no power. The heat is, in reality, even greater than on the plains, because it has not been intercepted by vapor-laden air. But the air itself is not heated. Owing to its extreme rarity and dryness, it neither impedes the passage of the sun's heat to the earth, nor prevents the return of that heat from the earth by radiation or reflection; and this very fact, that it does not impede the passage of heat, means nothing else than that the air does not become heated.*

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* The following passage, quoted by Prof. Tyndall from Hooker's "Himalayan Journals," illustrates the peculiarities referred to above: "At 10,000 feet, in December, at 9 A. M., I saw the mercury mount to 132°, while the temperature of shaded snow hardly was 22°. At 13,100 feet, in January, at 9 A. M., it has stood at 98°, with a difference of 68.2°, and at 10 A. M. at 114°, with a difference of 81.4°, while the radiating thermometer on the snow had fallen at sunrise to 0.7°." Such observations as these are well worth studying. It is interesting to consider that at the summit of the highest peaks of the Himalayas the midday heat of the sun must sometimes be near if not above the boiling point corresponding to those places, since water would boil on Mount Everest at a temperature of little more than 160°.

We have, then, so far as a rare atmosphere is concerned, two points to dwell upon — the readiness with which such an atmosphere permits the sun's heat to reach the surface of a planet, and the readiness with which it permits the planet's heat to pass away into space. Now, we might feel doubtful which of these two effects was chiefly to be regarded, were it not that on our own earth we have experience of the effects of a very rare atmosphere. We know that the climate of very elevated regions is relatively much cooler than that of places on the plain. Thus we learn that the direct heating powers of the sun are not so much to be considered, in judging of the climate of any region, as the quality of the atmosphere.

Yet we must not deceive ourselves by inferring that mere rarity of atmosphere can compensate fully for an increased intensity of solar heat. It is not true that the climate of a place on the slopes of the Andes or the Himalayas corresponds to that of a region on the plain which has an atmosphere equally warm. The circumstances are, in fact, wholly different. On the plain there is, it is true, the same amount of heat in the case supposed: but the air is denser and more moisture-laden; the nights are warmer because the skies are less clear and the heat escaping from the earth is intercepted by clouds or by the transparent aqueous vapor in the air; and, lastly, there is not so great a contrast between the warmth of the air and the direct heat of the solar rays.

If the atmosphere of Mercury, therefore, be excessively rare, as some have supposed, so as to afford an Alpine or Himalayan climate in comparison with the tremendous heat we should otherwise ascribe to the climate of the planet, there would by no means result a state of things resembling that with which we are familiar on earth. We must not, in our anxiety to people Mercury with creatures such as we know of, blind ourselves to the difficulties which have to be encountered. We cannot thin the Mercurial air, without adding to the direct effects of the sun upon the Mercurial inhabitants. Whether in this way we increase the habitability of the planet may be doubted when we consider that the direct action of the sun's rays upon the tropical regions of Mercury, thus deprived of atmospheric protection, would produce a heat four or five times greater than that of boiling water. It will hardly be thought that the intense cold in the shade, or during the Mercurial night, would compensate for so terrible a heat. In fact, this view of the Mercurial climate would lead us to find a close resemblance between the inhabitants of the planet and the unfortunates described by Dante as doomed

> "A sofferir tormenti e caldi e gieli."

It would seem hard to believe in the existence of any organized forms under such conditions, unless perhaps such "microscopic creatures, with siliceous coverings," as Whewell proposed to people Venus with.

However, we have yet to consider whether an atmosphere of a different sort might not be better suited to the requirements of Mercury. We have seen the effects of a rare atmosphere, let us inquire into those which might be ascribed to a dense one.

The ordinary effect of a dense atmosphere we know to be an increase of heat, which is certainly not what we require in the case of Mercury. Nor are we familiar with any region upon our earth in which a dense atmosphere produces a contrary *climatic* effect; so that we have no analogy to support us in the belief that, possibly, a dense atmosphere might, under particular circumstances, serve to guard a planet from the solar rays. It seems possible, however, that an atmosphere might be so constituted as to remain almost constantly loaded with heavy cloud-masses. In this case, it by no means follows that such effects would follow as we ordinarily associate with a moisture-laden atmosphere. Up to a certain point, doubtless, the increase of moisture in the air tends to an increase of warmth; because the aqueous vapor exercises a greater effect in preventing the escape of heat from the earth than in guarding the earth from the solar rays. And, as I have said, the only *climatic* effect we can associate with the frequent presence of large quantities of aqueous vapor in the air, or therefore with an ordinarily clouded state of the sky, is that of a general increase of heat. But, just as we know that a cloudy day is not necessarily nor even commonly a warm day, it may well be that an atmosphere so dense as to be at all times cloud-laden serves as a protection from the sun's intense heat. So that, instead of assigning dense atmospheres exclusively to the more distant planets, as some astronomers have done, we might be led to see in an envelope of great density the means of defending the inhabitants of Mercury and Venus from the otherwise unendurable rays of their near neighbor, the sun.

Although Mercury is not a planet which can be satisfactorily examined with the telescope, yet, so far as can be judged from his aspect, his atmosphere is in reality much denser than our earth's, and loaded with cloud-masses of enormous extent. Still the evidence on these points is far from satisfactory; and there is one peculiarity of the planet which does not accord with this view of the constitution of his atmosphere. Undoubtedly, if the light we receive from Mercury came from a cloudy envelope, it would be more brilliant than the light we should receive from the surface of continents and oceans. In fact, the most brilliant light we could receive from a globe of a given size, placed at a given distance from the sun, would be that which would be reflected were such a globe covered with clouds. Now, there can be no doubt whatever that Mercury does not reflect the same proportion of light from his surface that some of the planets do. He would be, when favorably situated, the brightest of all the planets, were this so;[*] though, seen as he always is, on the bright background of a full twilight sky, he would not make so striking an appearance as Jupiter does when in opposition. This, however, is not the case. I remember being much struck by the superior light of Jupiter, on the afternoon of February 23, 1868, when the two planets were very close together, Mercury being nearly at his brightest, whereas Jupiter, then near conjunction, was considerably less

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[*] Placing Mercury in perihelion and at his elongation, we get a half disk, the planet about 90,000,000 miles from us, and about 30,000,000 from the sun, his diameter about 3,000 miles. Now, if we wish to compare the light he then sends us, with that of Jupiter at his brightest, on the assumption of equal reflective powers, we must take Jupiter at a distance of about 360,000,000 miles from us, and about 450,000,000 miles from the sun, showing a full disk, his diameter about 90,000 miles (I put all the numbers *round*, for convenience of calculation). We find, then, that the ratio of Mercury's light to Jupiter's is

$$\frac{1}{2}\,\frac{(3{,}000)^2}{(90{,}000{,}000)^2\times(30{,}000{,}000)^2} : \frac{(90{,}000)^2}{(360{,}000{,}000)^2\times(450{,}000{,}000)^2}$$

or $\frac{1}{2}(4)^2\,(15)^2 : (30)^2$, or exactly 2 to 1.

The observation above cited is sufficient to prove that a very different state of things actually prevails: in other words, that the reflective powers of the two planets are very different. Unless, indeed, Jupiter shines in part by inherent light.

bright than when in opposition. Venus was close by, and outshone both Mercury and Jupiter.

It seems difficult, therefore, to believe that the light of Mercury comes from a cloudy envelope. But there is still one supposition which may restore our belief in the habitability of the planet by creatures not very different from those which inhabit our earth. If it has a double cloud-envelope, the upper like our cirrus clouds, less compact than the lower, and permitting a portion of the sunlight to pass through, it is possible that the lower cloud-layer would be seen partly in shadow. I must admit that the explanation is not quite satisfactory, because, just as much light as the outer clouds intercepted they would reflect; still, it is conceivable that the usual arrangement of these clouds may be such, that to us, who do not look at the planet in the direction in which the sun's rays fall, but somewhat aslant, the shadows of the upper clouds upon the dense and compact lower envelope may be rendered in large part visible.

After all, the reader may prefer the view which recognizes in the polar regions of Mercury places suitable for organic existences, while the equatorial and neighboring regions are zones of fire, whose dangers the bravest Mercurials, the very Livingstones upon that planet, would not dare to face. We may picture to ourselves, on this view, the various contrivances by which the inhabitants of the two polar (that is, in reality, temperate) circles manage to communicate. There may be regions where favoring circumstances narrow the uninhabitable zone so much that the inhabitants of one polar circle may travel to the other (or, at least, cross the most dangerous portion of the hot zone) in the course of the Mercurial night. Or perhaps tunnels may be run, or sheltered cuttings made, along which the voyage may be made in comparative safety. Ocean communication there can be none, if the Mercurial skies are clear, since the sun's heat on the tropical zone would suffice to boil away any water which might find its way there.

Certainly, the smallness of the planet and the diminished effects of gravity upon its surface would tend to make communication much easier, and the construction of protective tunnels or cuttings a comparatively light task. What the exact force of gravity at the surface of Mercury may be we do not know, because our means of determining the mass of the planet are not so satisfactory as in the case of the other primary members of the solar system. If Mercury had a satellite, we could tell his weight at once. If he were as large as Venus, we could tell his weight by observing his effect in disturbing the motions of that planet. As it is, the only means we have of weighing Mercury is the observation of his effect in disturbing any comet which may pass near him. In this way the planet has been weighed, but the balance thus employed is not a satisfactory one altogether, because we are not quite certain how much of the disturbance of a comet when near Mercury is due to the planet's attraction. Formerly, it was supposed that the mean density of Mercury was equal to that of lead; but, from the perturbations of Encke's comet in Mercury's neighborhood, astronomers have been lead to the conclusion that the density of the planet is not more than one-sixth greater than our earth's. It follows that, as his diameter is little more than three thousand miles, our earth is about fifteen times as heavy as Mercury. Gravity at his surface is such, that a pound weight of ours would weigh rather less than seven ounces on Mercury. Hence the creatures which seem to us most unwieldy — the elephant, the hippopotamus, and the rhinoceros, or even those vast monsters, the mammoth, the mastodon, and the megatherium, which bore sway over our globe in far-off eras — might emulate on Mercury the agility of the antelope or the greyhound.

There can be no doubt that, where gravity acts so feebly, all engineering operations would be rendered very much simpler — bridges could have a wider span, and yet be stronger than our terrestrial ones, buildings could be loftier and yet be raised more easily, and transit of all sorts would be effected much more readily, while at the same time the distances to be traversed are very much less than on our earth, since the surface of Mercury is little more than one-seventh of the earth's.

The peculiarities which characterize Venus are for the most part similar in kind to those we have had to consider in the case of Mercury. But at the outset of our inquiries into the physical habitudes of this most beautiful planet, we must point to the striking resemblance which it bears, in some respects, to our own earth. So far, indeed, as telescopic and physical researches have yet led us, the planet Mars, as we shall presently see, appears to exhibit habitudes more closely corresponding to those we are apt to consider essential to the wants of living creatures. But in size, in situation, and in density, in the length of her seasons and of her rotation, in the figure of her orbit and in the amount of light and heat she receives from the sun, Venus bears a more striking resemblance to the earth than any orb within the solar system. In fact, there is no other pair of planets between which so many analogies can be traced as between Venus and the earth. Uranus and Neptune are similar in many respects, but they differ in at least as many. Jupiter and Saturn are, in a sense, the brother giants of the solar scheme, while the dwarf orbs Mars and Mercury present many striking points of similarity; but between neither of these pairs can we trace so many features of resemblance as those which characterize the twin planets Venus and Terra, while the features of dissimilarity in either pair are perhaps even more obvious than the points of resemblance. Had Venus but a moon as the earth has, we might doubt whether, in the whole universe, two orbs exist which are so strikingly similar to each other.

And here we may pause for a moment to consider one of the most perplexing enigmas that has ever been presented to astronomers. Are we indeed certain that Venus has no moon? The question seems a strange one, when it is remembered that year after year Venus has been examined by the most eminent modern observers, armed with telescopes of the most exquisite defining power, without any trace of a companion orb being noticed. Nor, indeed, can any reasonable doubts be entertained respecting the moonless condition of Venus, by those who appreciate the character of modern telescopic observations; and yet, if I had begun this paragraph by stating the evidence in favor of the existence of a satellite, I believe that nearly every reader would have come to the conclusion that most certainly the Planet of Love has an attendant orb. They are not amateur observers only, who have seen a moon attending on Venus, but such astronomers as Cassini and Short, the latter with two different telescopes and four different eye-pieces. Four times, between May 3 and 11, 1761, Montaigne saw a body near Venus, which presented a phase similar to that of the planet, precisely as a satellite would have done. From these observations M. Baudouin deduced for the new star a diameter of about two thousand miles, and a distance from Venus nearly equal to that which separates the moon from the earth. In March, 1764, again, Rödkier saw the enigmatical companion; Horrebow saw it a few days later; and Montbaron saw it in varying positions on March 15, 28, and 29. Lastly, Scheuten, who witnessed the transit of Venus in 1761, declares that he saw a satellite accompany Venus across the face of the sun. So that we cannot be greatly surprised that even so skilful an observer as the late Admiral Smyth was disposed to believe in the existence of a satellite of Venus. "The contested satellite is, perhaps," he remarked, "extremely minute, while some parts of its body may be less capable of reflecting light than others; and when the splendor of its primary and our inconvenient station for watching it are considered, it must be conceded that, however slight the hope may be, the search ought not to be relinquished."

There is little occasion to dwell upon Venus's moonless condition, because the inferior planets are much less affected by the want of a moon than a superior planet would be. The service rendered by our own moon, as a luminary of the night, is the least important work she does in our behalf. It is as the chief regulator of the tides that the moon befriends us most usefully. Now, Venus has no need of lunar tides. Assuming that she has oceans such as those which exist upon the earth, her solar tides must be about two and a half times as high as the solar tides raised in our own oceans. And since our lunar tidal wave is about two and a half times as high as the solar one, we have tides ranging between the highest spring tides, which are three and a half times as high as the solar tide alone, and the lowest neap tides, which are only one and a half times as high as the solar wave. Venus has constant tides, therefore, corresponding very closely to the mean tides on our own earth; and therefore perfectly well adapted to subserve all the purposes which our tides render us, only with less variety in their mode of operation. Mercury also has sufficiently high solar tides, supposing he has extensive oceans (which may reasonably be questioned), since the smallness of his dimensions, tending of course to diminish the difference of action on which the sun's tidal influence depends, is fully compensated by his great proximity to that orb.

Venus has a year of two hundred and twenty-four days, seventeen hours, very nearly, and her distance from the sun, which varies little during the course of a year, is somewhat less than three-fourths of that which separates the sun from us. Her day is about thirty-five minutes shorter than ours, and her globe somewhat smaller than the earth's.

It is clear that, merely in the greater proximity of Venus to the sun, there is little to render at least the larger proportion of her surface uninhabitable by such beings as exist upon our earth. The sun, as seen in her skies, has a diameter one-third larger than he presents to us; and his apparent surface-dimensions, on which, of course, his heating and illuminating powers depend, are greater in the proportion of about sixteen to nine. This undoubtedly would render his heat almost unbearable in the equatorial regions of Venus, but in her temperate and subarctic regions a climate which we should find well suited to our requirements might very well exist; while her polar regions might correspond to our temperate zones, and be the abode of the most active and enterprising races existing upon her surface.

Here, however, we have been supposing that Venus has seasons resembling our own in character,— in other words, that her axis of rotation is inclined at about the same angle to the plane in which she travels. Observations have been made, according to which a very different state of things would appear to prevail. It has been said, on the authority of observers of some eminence, that her axis is inclined only 15° to the plane of her orbit.* If this is really the case, a number of singular and somewhat complicated relations are presented, the result of which it may be interesting to exhibit to the reader — especially as there is very little doubt that in the case of Uranus an axial peculiarity of this sort actually exists.†

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* Why is it that, in so many works of popular astronomy, the mistake is made of giving the inclination of a planet's equator to the orbit as the inclination of the axis to that plane? In nine out of ten astronomical works, the inclination of the earth's axis to her orbit is given as 23½°; were this the case, the larger part of the earth would be uninhabitable.

† If the observation of De Vico may be trusted, the inclination of Venus, though less than 75°, is still so considerable (about 55°) as to justify the general conclusions deduced in the following paragraphs.

In the first place, the arctic regions of Venus extend within fifteen degrees of her equator (if the axis is really bowed as supposed), while the tropics extend within fifteen degrees of her poles — so that two zones, larger by far than the temperate zones of our earth, belong both to her arctic and to her tropical regions. It is difficult to say whether her equatorial, her polar, or her arctico-tropical regions would be, to our ideas, the least pleasing portion of her globe.

An inhabitant of the regions near either pole has to endure extremes of heat and cold, such as would suffice to destroy nearly every race of living beings subsisting upon the earth. During the summer, the sun circles continually close to the point overhead, so that, day after day, he pours down his rays with an intensity of heat and of light exceeding nearly twofold the midday light and heat of our own tropical sun. Only for a short time, in autumn and in spring, does the sun rise and set in these regions. A spring or autumn day, like one of our days at those seasons, lasts about twelve hours; but the sun attains at noon, in spring or autumn, a height of only a few degrees above the horizon. Then presently comes on the terrible winter, lasting about three of our months, but far more striking in its characteristics even than the long winter night of our polar regions. For, near our poles, the sun approaches the horizon at the hour corresponding to noon; and though he does not show his face, he yet lights up the southern skies with a cheering twilight glow. But during the greater part of the long night of Venus's polar regions, the sun does not approach within many degrees of the horizon. Nay, he is farther below the horizon than the midnight sun of our arctic regions. Thus, unless the skies are lit up with auroral splendors, an intense darkness prevails during the polar winter, which must add largely to the horrors of that terrible season. Certainly, none of the human races upon our earth could bear the alternations between these more than polar terrors and an intensity of summer heat far exceeding any with which we are familiar on earth.

Let us see whether the equatorial regions are more pleasing abodes.

In these parts of Venus there are two summers, corresponding to the spring and autumn of the polar regions. At these seasons, the sun rises day after day to the point overhead, and the weather corresponds for a while to that which prevails in the tropical regions of our own earth. But between these seasons the sun passes away alternately to the northern and southern skies. During the season corresponding to summer, he is above the horizon nearly throughout the twenty-three and a third hours of Venus's day;* but he attains no great elevation, travelling always in a small circle close around the northern pole. During the season corresponding to winter, he is above the horizon only a very short time each day,† and is always close to the south, attaining only an elevation of a few degrees at noon. Thus we have the following curious succession of seasons: At the vernal equinox a summer much warmer than our tropical summers; about fifty-six days later, or at the summer solstice, weather resembling somewhat the spring of our temperate zones, only that the night is exceedingly short; yet fifty-six days later there is another summer, as terrible as the former; and lastly, at the winter solstice, the days are shorter and the cold probably more intense than in the winter of places near our arctic circles. In such regions the contrasts, rather than either of the extremes of climate, would be most trying to terrestrial races; and it is scarcely too much to say that no races subsisting upon our earth could possibly endure such remarkable changes, succeeding each other so rapidly.

Lastly, the beings who inhabit the wide zones which are at once tropical and arctic have climates ranging between the two limits just considered. If they are near the equatorial regions, they suffer from all the vicissitudes of the equatorial climate, with this further tribulation, that, in midwinter, they do not see the sun even at midday, a circumstance by no means compensated (according to our ideas) by the fact that near the summer solstice the sun does not set. If they are near the polar regions, they have a summer even more terrible than the polar summer, and a winter scarcely less dreary and bitter.

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* On the equator itself, as on our own, the day is always equal in length to the night. The above account corresponds to a place near the borders of the equatorial zone.

† In Admiral Smyth's "Celestial Cycle," the only work in which, so far as I am aware, the effects of the inclination ascribed to Venus's axis have been at all considered, it is stated that in the year of Venus there are but nine and a quarter of her days, "reckoned by the sun's rising and setting, owing to which the sun must appear to pass through a whole sign in little more than three-quarters of her natural day." He does not give any reasons for this remarkable statement, which most certainly is not correct. In all places outside the arctic circles of Venus, the year contains as many natural days as there have been rotations of Venus, wanting one only (as in the case of our own earth): in the remaining regions there will be more or fewer days, according as the station considered is nearer to or farther from the arctic circle. Smyth's remark that the varying amplitude of the sun (his distance, that is, from the east and west points), at rising or setting, would give travellers on Venus readier means than our seamen have, of determining the longitude, is just. But the problems involved must be very difficult, and I wish her mathematicians joy of them. The cadets in our schools and training-ships have an easy time of it, compared with the unfortunate beings who are to officer the ships of Venus — always supposing her axis is inclined as we have been assuming.

Fortunately for our belief in the habitability of Venus, astronomers are far from accepting with confidence the assertions of those observers who have assigned to Venus an inclination so remarkable. If her inclination should at all resemble the earth's, there is every reason to believe that her physical habitudes also resemble those of the earth. In this case, the argument from analogy, presented in the opening chapter of this work, seems to force upon us the conclusion that she is inhabited; while we may believe, though perhaps with less confidence, that a close resemblance subsists between the creatures which people her surface and those with which we are acquainted.

We have no direct evidence, indeed, on which to ground our belief that the greater proximity of Venus to the sun may not be accompanied by any very remarkable peculiarities in the characteristics of her climate. But we have an indirect argument of some strength. If Venus is much nearer than the earth to the sun, the earth, in turn, is much nearer to the sun than Mars is. Yet, as we shall see in the next chapter, we have clear evidence from telescopic observation, and still clearer evidence as the results of spectroscopic research, that the climatic arrangements on Mars do not differ in any remarkable degree from those of our own earth. It would follow, therefore, as at least probable, that a similar resemblance prevails between the climate of the earth and that of Venus. So that, despite the claim which Dr. Whewell has put in for microscopic animalcules with siliceous coverings as the sole inhabitants of Venus, I can find no reason (if the abnormal axial inclination above considered is once disproved) for denying that she may be the abode of creatures as far advanced in the scale of creation as any which exist upon the earth.

Gravity at the surface of Venus is so nearly equal to terrestrial gravity, that the difference is altogether insufficient to introduce any noteworthy effects. The delicate adjustment of the sap-passages of plants to the force of terrestrial gravity, which Dr. Whewell notices in his "Bridgewater Treatise," might indeed be disturbed, if the earth's gravity were suddenly made equal to that of Venus. But it would be strangely to limit our conception of Nature's powers of adaptation, to suppose that therefore there can be no vegetation on Venus resembling that with which we are familiar.

Venus is the only planet the extent of whose atmosphere has been carefully estimated. If Venus had no atmosphere, she would present, when horned, a semicircular convexity; whereas the refractive effects of an atmosphere, by causing the sun to illumine rather more than a full hemisphere, would tend to lengthen her horns. It has been found that her convexity when she is horned exceeds a semicircle, and, from the observed extent of this excess, it has been calculated that her atmosphere is so far more extensive than ours as to make its refractive effects on a body near the horizon about one-third greater. So that, as this is about the proportion in which the diameter of the sun as seen from Venus exceeds that which he presents to us, the inhabitant of Venus, like the inhabitant of our earth, sees the sun fully raised above the horizon at the moment when, but for refraction, his orb would be just concealed beneath it.

Of the constitution of the atmosphere of Venus we know little. The spectrum of her light shows the dark lines which belong to the solar spectrum, and the Padre Secchi has noticed certain faint lines, which seem to indicate the presence of aqueous vapor in the atmosphere of the planet. But he scarcely gives satisfactory evidence that the lines he has thus seen were not due to the absorption exercised by aqueous vapor in our own atmosphere. The same observer finds, in the strengthening of the nitrogen lines near the F line of the spectrum, evidence that the atmosphere of Venus is constituted very similarly to the air we breathe.

On the whole, the evidence we have points very strongly to Venus as the abode of living creatures not unlike the inhabitants of earth. With the sole exception of the inclination, which has been, without sufficient evidence, assigned to the planet's equator, I can see nothing which can reasonably be held to point to an opposite conclusion. Certainly the strong light which the sun pours upon Venus need least of all be objected to, since, if there is one adaptative power which Nature exhibits more clearly than another, it is that by which the various creatures we are acquainted with are enabled to live in comfort under all degrees of light, from the obscurity in which the mole pursues his subterranean researches, to the blazing light of the noonday sun toward which (in fable, if not in fact) the eagle turns his unshrinking eyes.

There is one peculiarity which yet remains to be noticed. Many are disposed to find, in the beauty of the celestial objects which deck the skies of different planets, a certain proof that reasoning beings must exist who can appreciate the display. Surely the argument has very little force, since we know that myriads on myriads of ages must have passed, during which the glories of our own heavens were displayed, night after night, with none to regard them. The moon has passed through all her phases, the star of morning and of eve has shed its soft radiance upon the terrestrial landscape, Jupiter and Saturn have pursued their stately courses among the fixed stars, and the glories of those constellations which shine with equal splendor upon all the planets of the solar scheme have been displayed in all their unchanging magnificence, while as yet our earth was the abode but of hideous reptiles, or of yet more monstrous creatures in forest and in plain.

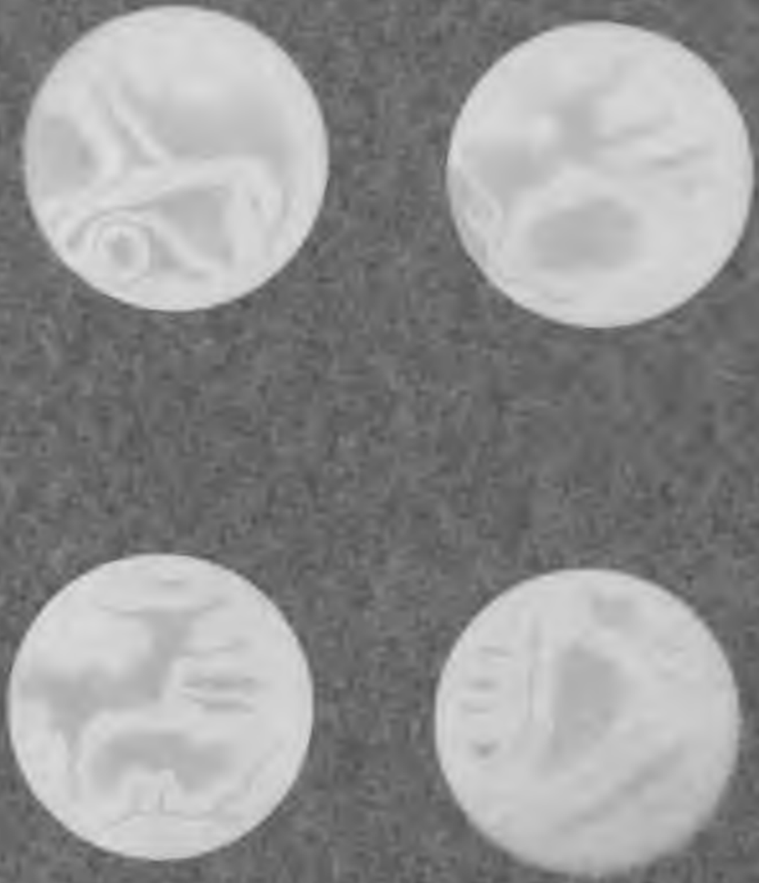
If this argument were really of force, doubtless there are no planets in the whole range of the solar system to which it might not be applied. Each has some special object of beauty in its heavens, which is not exhibited to the rest. Certainly Mercury and Venus are no exceptions to this rule. The inhabitant of Mercury sees in Venus an orb which, when favorably situated, far outshines in splendor the brightest of the planetary orbs seen in our skies. So far, indeed, as light-giving power is concerned, Venus must be no contemptible moon to the Mercurials when she is nearly in opposition. Our earth, too, with its companion moon, must form a noble object in the sky of Mercury, though, without telescopic aid, the moon perhaps may not be separately visible. To the inhabitants of Venus, Mercury and the earth must be splendid objects. The former would not only appear much larger than to ourselves, but, being seen almost as favorably as we see Venus, would form a much more striking object in the morning or evening sky of that planet. The earth, as seen by the inhabitants of Venus, must shine much more splendidly than Jupiter does in our skies. Our moon must be distinctly visible, so that, without the aid of any telescope, the inhabitant of Venus has such evidence of the Copernican theory as would suffice, if properly handled, to rout the ranks of the Ptolemaists, supposing there have ever been people in Venus foolish enough to imagine the tiny globe they live upon to be the centre of the universe.

CHAPTER IV.

MARS, THE MINIATURE OF OUR EARTH.

IT is singular that, among all the orbs which circle around the sun, one only, and that almost the least of the primary planets, should exhibit clearly and unmistakably the signs which mark a planet as the abode of life. We have examined Mercury and Venus, the only other orbs which belong, like the earth and Mars, to the scheme of the minor planets, and we have found little to guide us to any certain conclusion respecting their physical habitudes. When we pass beyond the wide gap which separates the minor planets from the giant members of the solar family, we shall find much to attract our admiration, much to force upon us the belief that these orbs have been created to be the abodes of even nobler races than those which subsist upon our earth; but we shall find little to justify us in asserting that they resemble the earth in those habitudes which seem essential to the wants of terrestrial races. The planet Mars, on the other hand, exhibits in the clearest manner the traces of adaptation to the wants of living beings such as we are acquainted with. Processes are at work out yonder in space which appear utterly useless, a real waste of Nature's energies, unless, like their correlatives on earth, they subserve the wants of organized beings.

I would not indeed insist, as some have done, too strongly upon this argument. I know that on every side we see tokens of an exuberant activity in Nature, which, according to our ideas, may appear to savor of wastefulness. The cloud which has been raised by the solar energies from tropical seas, and which the winds have wafted over continents, may shed its waters on the sea or in the desert, where seemingly they are wholly wasted. Winds may spend their force apparently in vain. And in a thousand ways Nature's busy forces may be at



THE PLANET MARS
(Downing.)

work where we, in our short-sightedness, can see no useful purpose which they subserve.

But there is a marked distinction between such apparent instances of wasteful action, and the systematic processes which are taking place over the globe of Mars.

Little as we can appreciate the real character of Nature's work upon our earth, we can yet dimly trace out a necessity (depending upon the order which actually exists) for that which yet appears to resemble waste. We see, for instance, that if a country or a continent is to be provided with a due supply of rain, without supernatural intervention at every step of the process, that result can only be secured by what may be described as a random distribution, involving always what to us resembles waste. If, out of a thousand showers, ten only fall so as to be useful to the land, the object of Nature is subserved, and the useful rainfalls serve to explain the seemingly wasted ones. In reality, of course, there has not been a random distribution, nor has there been any waste; I infer, merely, that a sort of purpose is, in such a case, dimly seen, even by man, who can see so short a distance into the workings of the Almighty.

But in the case of Mars we have no such explanation of the processes we observe, if we dismiss our belief that he is the abode of living creatures. For if Mars be, indeed, untenanted by any forms of life, then these processes going on year after year, and century after century, represent an exertion of Nature's energies which appears absolutely without conceivable utility. If one cloud, out of a hundred of those which shed their waters upon Mars, supplies in any degree the wants of living creatures, then the purport of those clouds is not unintelligible; but if not a single race of beings peoples that distant world, then indeed we seem compelled to say that, in Mars at least, Nature's forces are wholly wasted. Such a conclusion, however, the true philosopher would not care needlessly to adopt.

Let us consider what astronomy has taught us respecting the ruddy planet.

The globe of Mars is about five thousand miles in diameter, so that his linear dimensions bear to those of the earth the proportion of about five to eight. His surface, therefore, is less than that of the earth in the proportion of about twenty-five to sixty-four, or, more exactly (and more conveniently), the surface of the earth is two and a half times as extensive as that of Mars.

The substance of Mars has an average density rather less than three-fourths of our earth's, or very nearly four times that of water. Thus gravity at his surface is much less than terrestrial gravity. It is, in fact, even less than gravity at the surface of Mercury, insomuch that one of our pound weights placed at the surface of Mars would weigh but 6 ozs. 3 dwts., instead of nearly seven ounces as on Mercury. I have already dwelt on the effects of such a relation as this, and shall have occasion, when describing the habitudes of Jupiter, to discuss the converse relation. But I may remark, in passing, how singular it is that we should be compelled to people the smallest planets with the largest inhabitants, if we wish to bring the inhabitants of different orbs to about the same scale of activity. A Daniel Lambert on Mars would be able to leap easily to a height of five or six feet, and he could run faster than the best of our terrestrial athletes. A man of his weight, but proportioned more suitably for athletic exercises, could leap over a twelve-foot wall. On the other hand, a light and active stripling removed to Jupiter would be scarcely able to move from place to place. On the sun his own weight would simply crush him to death.

Mars travels in an orbit of considerable eccentricity; in fact, the centre of his orbit is no less than thirteen millions of miles from the sun. Accordingly, the light and heat he receives from that luminary vary to an important extent. In fact, he gets about half as much heat and light again when in perihelion as when in aphelion. This circumstance affects to an important extent the climatic relations of his two hemispheres, as we shall presently see.

When Mars is at his mean distance from the sun, the light and heat he receives are less than ours in the proportion of about four to nine. The length of his year also constitutes a noteworthy circumstance in which his habitudes differ from those of our earth. His year contains very nearly six hundred and eighty seven of our days, so that each of the Martial quarters lasts about five and two-thirds of our months. But, owing to the eccentricity of his orbit, the winter and summer of the northern and southern hemispheres are not equal. The Martial day is nearly forty minutes longer than ours.*

His equator is inclined at an angle of about twenty-seven and a quarter degrees to the plane of his orbit, and as the corresponding inclination in the case of the earth is about twenty-three and a half degrees, it will be seen that his seasonal changes do not differ much in character, so far at least as they depend on inclination, from our own.

The axis of Mars is so situated that the summer of his northern hemisphere occurs when he is at his greatest distance from the sun. The same relation holds in the case of the earth, the sun being one million five hundred thousand miles nearer to us in winter than in summer, whereas, to those who live in the southern hemisphere, he approaches nearer in summer than in winter. But the effects resulting from the relation in the case of Mars must be very much more striking than those we recognize. For, whereas the sun gives only one-fifteenth more heat to the whole earth in January than he does in July, the sun of Mars gives half as much light again in perihelion as in aphelion. The summer of the northern hemisphere of Mars must be rendered much cooler and the winter much warmer by this arrangement. On the other hand, the contrast between the summer and winter of the southern hemisphere is rendered more striking than it otherwise would be.

It is, however, the telescopic aspect of Mars, rather than relations such as we have been dealing with, that affords the most interesting evidence respecting the fitness of the planet to be the abode of living creatures. Although the least but one among the primary planets — a mere speck compared with Jupiter and Saturn — Mars has been examined more minutely and under more favorable circumstances than any object in the heavens except the moon. He does not approach us so closely as Venus, nor does his disk appear so large as Jupiter's, yet he is seen more favorably than the former planet, and on a larger scale, in reality, than the latter. In fact, whereas Venus is one of the most unsatisfactory of all telescopic objects, Mars is one of the most pleasing; and, whereas Jupiter is always more than three hundred and eighty millions of miles from us, Mars sometimes approaches us within less than forty millions of miles.

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* More exactly, the length of the Martial day is 24h. 37m. 22.735s. This estimate I have obtained by comparing pictures taken by Hooke in 1666, and by Dawes and Browning in 1866–1869 — with precautions sufficing to secure that no complete rotation should anywhere be lost sight of.

Yet even this distance is enormous, and it affords high evidence of the skill with which modern telescopes are constructed and used, that astronomers should have been able to span that mighty gulf, and to bring from beyond it reliable information respecting the structure of so distant a world.

Such information has been brought, however, and is full of interest.

Viewed with the naked eye, the most remarkable feature Mars presents is his ruddy color. In the telescope this color is not lost, but, instead of characterizing the whole surface of the planet, it is confined to particular regions — the intermediate parts being for the most part darker, and of a somewhat greenish hue. But a noteworthy feature adds largely to the beauty of the picture presented by the globe of Mars. Two bright spots of white light are seen on opposite sides of his disk, presenting precisely such an appearance as we might imagine the snowy poles of our earth to exhibit to an astronomer on the planet Venus.

Toward the edge of the disk, the ruddy and the greenish tracts are lost in a misty whiteness, which grows gradually brighter up to the very border of the planet. We shall presently see that this peculiarity, rightly understood, is one of the most instructive features of the planet's aspect.

No telescopist has yet been able to recognize a satellite attending on the Planet of War.

It was discerned, more than two hundred years ago, that the reddish spots on Mars, and the darker regions which lie between them, are not accidental or variable phenomena, but represent permanent peculiarities of the Martial surface. Cassini, with one of those outrageously long telescopes which were used before the invention of achromatic refractors, was the first to discover this. But the ingenious Hooke seems to have obtained better views of Mars in 1666. At least, his pictures of the planet are the only ones taken in the seventeenth century, in which I can recognize the now well-known aspect of the Martial continents and oceans.

Since then, Maraldi and the Herschels, Arago, Secchi, Kunowski, Beer, and Mädler, and a host of other eminent astronomers, have not thought the study of the planet's aspect beneath their notice. Within the last few years, also, this work has been prosecuted by Nasmyth and Jacob, Delarue and Phillips, and finally and most successfully by Lockyer and Dawes. The last-named observer, especially, whose acuteness of vision earned for him the title of the eagle-eyed, took so many and such admirable views of the planet as to render it possible to form a globe of Mars. Sir William Herschel had charted the planet, and Messrs. Beer and Mädler had made improved Martial maps; while Prof. Phillips, from observations made by himself and Mr. Lockyer, had constructed two globes of Mars in which many features were presented. But Mr. Dawes's pictures of the planet were sufficient, when carefully compared, for the formation of a globe in which no large area of the planet should be left bare of details. He intrusted to me no less than twenty-seven drawings of Mars, the choicest specimens of a very large series, that I might chart the planet from them. Four of his drawings are shown in the accompanying plate. They are so selected, that the features just coming into view in one are just passing away in the next. The accompanying chart of Mars, in which the darker parts of the planet are assumed to be seas, and the reddish tracts continents, exhibits the results obtained from the study of the complete series. This chart is on the stereographic projection, and is inverted — the south polar regions, that is, are at the top — because the telescopes commonly used by observers exhibit inverted views of the celestial objects.* At the top of the map we see the icy region which lies at the southern pole of Mars. Around that region is a sea unnamed in the map. Then along the southern temperate zone there lie several tracts of Martial land, named after Cassini, Lockyer, and other astronomers. These regions appear to form a continuous land-belt round the temperate zone; though there is some uncertainty on this point, owing to the fact that the coast-line is not often very distinctly visible. We now approach, however, a part of the map where all the features are thoroughly recognized and permanent. Next to the circle of

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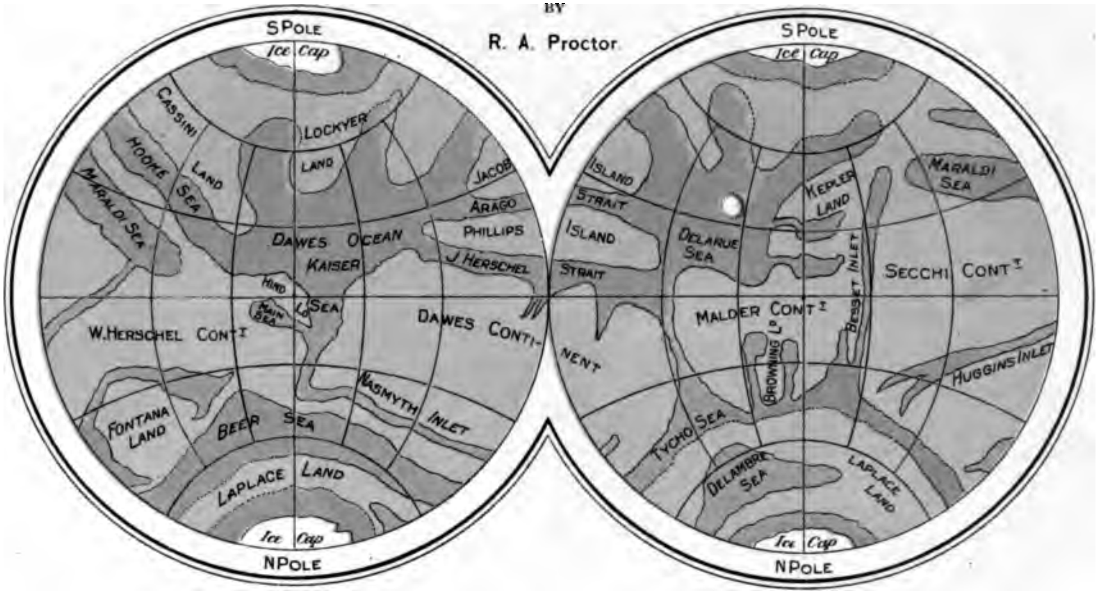
* Mr. Browning, F. R. A. S., has formed a globe of Mars from my chart, and publishes an interesting series of photographs of this globe which give fine stereoscopic effects.

A CHART OF MARS,

Laid down on the Stereographic Projection,

BY

R. A. Proctor.



From Drawings by Dawes.

point into Beer Sea, and at another into Tycho Sea. Then comes a zone of land, called Laplace Land, in which lies an enormous lake called Delambre Sea. Next is a narrow zone of water called the Schröter Sea, and so we reach the north-polar ice-cap.

I have been speaking of the spots on Mars as though they undoubtedly represented land and water. But many may be disposed to question the evidence we have on this point — to ask why the ruddy spots should be held to be continents or islands, and the greenish-colored markings to be oceans, seas, and lakes. We know that, for a long time after the invention of the telescope, astronomers called the darker portions of the moon, *seas*. They spoke of the Sea of Serenity, the Sea of Crises, the Sea of Humors, and so on, and we now know for certain that these dusky regions are not seas. It may be asked, therefore, how we can feel certain that the dark spots on Mars are oceans.

At first sight, this question seems a difficult one to answer. The most powerful telescopes have been directed toward the moon, without affording any satisfactory information respecting the condition of its surface. Mars, therefore, which lies — even under the most favorable circumstances — more than one hundred and sixty times farther from us than the moon, might be thought to be altogether beyond the reach of our telescopists — so far, at least, as any knowledge of the Martial surface is concerned. But one important distinction between Mars and the moon must be carefully attended to. The surface of the moon is always the same — no natural processes seem ever to take place over that scene of desolation, though the moon is exposed to contrasts of temperature, compared with which the distinction between the intensest heat of our summers and the bitterest cold of our winters seems altogether evanescent. But, on Mars, the case is certainly different. Whatever opinion we may form respecting Martial habitudes, whether we assume or not that Mars is the abode of any forms of animal life, there can be no question whatever that physical processes of change are taking place on a grand scale in that distant world. Many evidences of this can be at once adduced. We have spoken of the Martial features as constant. They differ, for instance, from the markings on Jupiter, which are as changeful as the aspect of our

April skies. But though the same marking may have been seen by Hooke in 1666, by Maraldi in 1720, by Herschel in 1780, by Beer and Mädler in 1830-'37, and by Dawes in 1852-'65, yet it by no means follows that it is always visible when the part of Mars to which it belongs is turned toward us. A veil is sometimes drawn over it for hours or even days together. And this veil has nothing to do with the distinctness or indistinctness with which our own atmosphere permits us to see the planet. A spot will be blurred and indistinct when a neighboring marking is exhibited with unusual clearness.

Let us consider an instance of this peculiarity. On October 3, 1862, Mr. Lockyer was observing Mars late in the evening. He noticed that a part of Dawes Ocean, where it borders on Herschel Continent, was hidden from view. In place of the ordinarily dark aspect of this region, a faint, misty light, with ill-defined borders, was observable. As the evening progressed, he noticed that the outlines gradually became clearer, but, when he gave up observation (at about half-past eleven), the white light still continued to veil the outline of a part of Dawes Ocean. Now, Mr. Dawes observed Mars on the same night, at a quarter-past twelve. The drawing which he took at that hour shows that the process of clearing up, noticed by Mr. Lockyer as being in progress in the earlier part of the night, had, by the time Mr. Dawes began work, entirely lifted off the veil which concealed the coast-line. The remains of the misty light seen by Lockyer are still to be detected in Mr. Dawes's drawing, but they have passed farther south, and no longer hide the shores of Dawes Ocean.

The Padre Secchi, of the Collegio Romano, states that he has often noticed similar apearances, while observing Mars with the fine refractor in the observatory of that institution.

But yet another peculiarity of the same sort remains to be mentioned. Mars, as I have said, has his winter and summer seasons. Since we know the position of the Martial equator upon his surface, we can tell what season is in progress in either hemisphere at any given time. Now, it has been noticed that, when it is winter in one hemisphere, and therefore summer in the other, the former hemisphere is nearly always hidden from view by just such a veil as I have spoken of above.

I may remark, in passing, that this peculiarity has led many observers to form very erroneous impressions respecting the distribution of land and water over the surface of Mars. Seeing one hemisphere covered for weeks together with whitish light, they have concluded that there are no oceans there; and if they have no other opportunity of observing the planet, the mistaken impression remains, and is published to the world with all the authority of the observer's name.

Now, what is this veil which, sometimes for a few hours or days, at others for months together, is drawn over the features of the Martial globe? Have we any terrestrial analogies, by means of which we may interpret this phenomenon?

To answer these questions, let us conceive the case of an observer on Venus, watching our earth. Would such an observer always see the features of this globe with equal distinctness? When heavy masses of cloud are drawn over a wide expanse of country — spreading often, as meteorologists record, for hundreds and even thousands of miles — can we suppose that the astronomer on Venus could pierce through the veil? Since we cannot see the bright body of the sun through a dense cloud-veil, we may be certain that the observer on Venus cannot see the oceans and continents of our earth when thus cloud-shadowed. So far as the cloud-veil extends, the lands and seas of this globe would be to him, at such a time, as though they were not.

Here, then, we have an argument from analogy for supposing that the veil, which from time to time conceals the Martial features, may resemble terrestrial cloud-banks. Let us next inquire whether there is any thing in the behavior of the Martial veil to justify this view.

It is clear that, if we held the concealing medium to be of a cloudy nature, the disappearance of the features of the hemisphere which is passing through the Martial winter would indicate that in winter the Martial skies are more clouded than in summer. We know that this is the case on our own earth — that fogs and mists, clouds, rain, and snow, are phenomena far more frequently observed in winter than in summer. We know also why it is so. The cold winter air is unable to retain the aqueous vapor continually passing into it, and is thus forced to precipitate this vapor in one or other of the forms just named. Nor can we see any reason why the Martial atmosphere, supposing it to resemble our own, should not act in precisely the same manner. Thus we recognize, in the remarkable seasonal peculiarity above described, what seems to be the exact counterpart of processes recognized upon the earth.

And though I admit that there is considerable objection to the mode of argument I am next going to make use of, yet, as it is one which has great weight with many minds, and is not without its own peculiar force, I feel justified in applying it as a subsidiary support to the views I am discussing. It is known that the peculiarities which characterize terrestrial atmospheric phenomena tend in an important manner to mitigate the extremes of summer and winter temperature. The clouds which hang over our winter skies, far from acting to increase the coldness of winter through their effect in keeping off the sun's rays, in reality represent an enormous supply of heat brought from warmer parts of the earth, and liberated for our benefit as the invisible vapor of water assumes the form of cloud or rain. And although these processes are strictly in accordance with natural laws, yet we are justified in recognizing them as evidences of the beneficence of the Almighty. Now, on Mars, we may be sure, the winters tend to be far more bitter than ours, partly because of his greater distance from the sun, but chiefly because of the more marked contrast existing between his various seasons. Hence, if there are living creatures on Mars, it can scarcely be doubted that an arrangement such as that which prevails on earth is yet more necessary to the welfare of the Martialists. Thus, we derive an argument from the *a priori* consideration of the nature of Martial requirements, to favor our interpretation of the phenomena actually observed.

Perhaps the reader may be disposed to inquire whether the clearing up of a portion of the Martial disk observed by Lockyer and Dawes admits of interpretation in a similar way. To this it may be replied that, from the observed position of the region in question, the Martial *time of day* there must have been somewhere about noon when Mr. Lockyer began his observations, and about one o'clock in the afternoon (according to our terrestrial mode of reckoning) when Mr. Dawes observed the planet. It is no uncommon thing to see our terrestrial skies clear up soon after midday; and if the veil which conceals the Martial features is really cloudy, this is precisely what happened out yonder, forty millions of miles away from us, on the day in question.

I think the reader will at least concede that the explanation here given of these peculiarities is more natural than one which was put forward some time since by an eminent French astronomer. He urged that Martial vegetation, instead of being green like ours, is red; hence in the Martial summer the surface, as seen by us, assumes a ruddy aspect, while the wintry hemisphere loses its ruddy tint. According to this interpretation, such changes as were noticed by Secchi would indicate the sudden blooming forth of Martial vegetation over hundreds of square miles of the Martial surface.

To the evidence already dealt with may be added that which is afforded by the whiteness of the disk of Mars near the edge. Knowing that the parts of Mars which thus appear concealed in mist are those where it is morning or evening to the Martialists, we see a close analogy here to terrestrial relations, since our own skies are commonly more moisture-laden in the morning and evening than near midday.*

I may here pause, in passing, to notice under what difficulties the observation of Mars is conducted by the terrestrial observer. To begin with, the sky must be exceptionally clear; and none but the practised observer knows how seldom there occurs what is called "a good observing night." Then it must be *a fine day for the Martialists,* for clouds over Mars, or even an imperfectly clear atmosphere, must produce quite as bad an effect in spoiling the definition of Martial features as similar phenomena on earth. Again, Mars only comes into a favorable position once in every two and a quarter years, continuing to be well placed for only a few months. Thus it happens that, although Mars has been telescopically observed for more than two hundred years, the actual time during which he has been favorably placed for observation has been very much less; and, taking into account all the requirements for good definition, it may be said that Mars has not been under really effective observation for more than a very few days.

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* In the *Popular Science Review* for January, 1869, I have indicated a subsidiary explanation of this peculiarity, founded on the probable shape of the Martial clouds. For the same reason that, near the horizon, our own cumulus clouds seem more closely packed than overhead, the Martialists would see a clearer sky overhead than near the horizon. It follows, at once, that we should see those parts of the surface of Mars best which we look upon in a nearly vertical direction, that is the central parts of his disk.

Of course, if we admit that the vaporous envelope which occasionally hides parts of Mars is aqueous, we must believe in the existence of oceans upon Mars. And, from our knowledge of the appearance of our own seas, we should immediately recognize the greenish parts of Mars as the Martial oceans, and look upon the ruddy parts as continents. We have seen that the behavior of the vaporous envelopes corresponds to that of our own clouds and fogs. But it might be thought possible that the vapors arise from fluids other than water; that, in fact, a state of things exists upon Mars wholly different from that which prevails upon our own earth.

Ten years ago it would have been very difficult to disprove such an argument as this, however *bizarre* it may seem. But the wonderful powers of the spectroscope have been applied to this question, and there is no mistaking the results which have been obtained. We must premise that this is hardly a favorable case for the application of spectroscopic analysis, which (as available to the astronomer) deals most effectively with self-luminous objects. Still, there was a possibility that the light which comes from Mars might have been so acted upon by vapors in the Martial atmosphere, that its spectrum would be affected in an appreciable manner.

Mr. Huggins examined Mars in 1864 without satisfactory results, but at the opposition of Mars in 1867 he was more successful. In the following description of his most striking observation I epitomize his account: On February 14th he examined Mars with a spectroscope attached to his powerful eight-inch refractor. The rainbow-colored streak was crossed, near the orange part, by groups of dark lines agreeing in position "with lines which make their appearance in the solar spectrum when the sun is low down, so that its light has to traverse the denser strata of our atmosphere." To determine whether these lines belonged to the light from Mars or were caused by our own atmosphere, Mr. Huggins turned his spectroscope toward the moon, which happened to be nearer the horizon than Mars, so that the atmospheric lines would be stronger in the moon's spectrum than in that of the planet. But the group of lines referred to was not visible in the lunar spectrum. Hence it was clear that they belong to the Martial atmosphere, and not to ours.

I have said that these lines appear in the solar spectrum when the sun is shining through the denser strata of our atmosphere. Let us consider a moment the light which this fact throws on the nature of the Martial atmosphere. It must contain at least those constituent vapors whose existence in our atmosphere causes the appearance of these lines in the solar spectrum. Hence there must be *some* similarity between the Martial atmosphere and our own. But we know from the researches of the Padre Secchi, that it is the aqueous vapor in our air which causes the appearance of the lines in question. Hence there must be aqueous vapor in the Martial atmosphere.

This discovery at once justifies the title of the present chapter. Let us consider what a number of interesting results follow from it.

The water in the Martial air must be raised from seas and rivers upon the planet. These, therefore, consist of water and not of other fluids. The two white spots, then, on the Martial disk are no longer doubtful appearances. Before the discovery that water exists on Mars, it was perhaps somewhat bold to pronounce that these spots certainly indicate the presence of ice-fields around the Martial poles, resembling those which exist around the poles of the earth. Sir William Herschel, indeed, with that confidence which he always showed when he had a trustworthy analogy to guide him, came to this conclusion on the strength of the correspondence between the changes of the two spots and the progress of the Martial seasons. But many astronomers felt that there was still room to doubt whether we could really speak of the spots as

> "The snowy poles of moonless Mars."

Now, however, we know that they can be no other than snow-caps. Nay, if Mars were so far off that we could not distinguish these spots, we could yet, on the strength of what the spectroscope has taught us, pronounce confidently that his polar regions must be icebound.

Let us proceed a step or two farther. We have seen that there are oceans on Mars; we know that clouds and vapors rise from those oceans and are wafted over his continents; and, finally, we have learned that snow falls on the Martial polar regions. These things are very interesting in themselves, but they indicate the occurrence of processes yet more interesting. The formation and the dissipation of clouds are among the most important of all the processes by which Nature arranges and modifies the temperature of our earth. The heat of the sun's rays is used up, so to speak, in raising aqueous vapor from the surface of the ocean. Thus the air is rendered cooler than it otherwise would be, and this takes place just where coolness is most needed. But the aqueous vapor, once raised, is swept by the winds to other regions. So long as the air remains warm, the aqueous vapor remains unchanged; but, so soon as it has been carried to colder regions, it is condensed into the form of cloud or mist, and while changing to this form it parts with the heat which had turned it into vapor. Thus where heat is in excess, it is used up in forming aqueous vapor, and where heat is wanted there the aqueous vapor distributes it.

We see, then, that on Mars there exists the same admirable contrivance for tempering climates which we find on our own earth.

But let us consider yet another office fulfilled by aqueous vapor. It not only serves to convey the heat from the warmer parts of the earth to those regions where heat is most needed. It forms clouds which serve to shelter the earth from the sun's heat by day, and to prevent the escape of the earth's heat by night, which also, in refreshing rains, "drop fatness on the earth." Now, the clouds on Mars are certainly dissipated in some way, because, as I have said, astronomers have repeatedly seen them disappear. And doubtless, like our own clouds, they are often dissipated by the sun's heat. But we may take it for granted that, like our terrestrial clouds, they are also often dissipated by falling in rain. Thus the Martial lands are nourished by refreshing rainfalls; and who can doubt that they are thus nourished for the same purpose as our own fields and forests — namely, that vegetation of all sorts may grow abundantly?

But yet, again, the transit of clouds from place to place implies the existence of aërial currents. Clouds cannot, indeed, even form and be dissipated without occasioning wind-currents; and it need hardly be said that the Martial clouds could not be carried to his polar regions, there to fall in snow, unless the atmospheric currents on Mars were extensive and persistent. We see, then, that Mars has winds as our earth has. Doubtless his trade-winds are less marked than ours, because his surface rotates less rapidly than the earth's, his globe being much smaller, while his rotation-period is slightly greater. But he has less need for trade-winds, his oceans being so much less extensive than ours. No Columbus on Mars has ever needed the persistent breath of easterly winds to encourage him on his voyage to an undiscovered continent. Rather, the intricate navigation of the narrow Martial seas would be favored by variable breezes. But the great purposes which the circulation of our own atmosphere subserves are carried out efficiently out yonder on Mars. The air is cleansed and purified, its thermal and electrical conditions are regulated, clouds are wafted from place to place; and, in fine, the atmosphere is rendered fit for all those purposes for which, like our own, it has doubtless been created.

We may trace yet further, however, the results which flow from the existence of aqueous vapor in the atmosphere of Mars. We see the polar snows aggregating in the Martial winter and diminishing in the Martial summer. And we know that, on our own earth, the increase and the diminution of the polar snows are processes intimately associated with the formation and maintenance of the oceanic circulation. Doubtless much yet remains to be done before that system of circulation will be fully understood. The rival views which have been maintained by Sir John Herschel and Captain Maury have served to throw a certain air of doubt over the theory of ocean-currents.* But whether we ascribe the equatorial currents of our oceans to the trade-winds with Herschel, or to differences of specific gravity with Maury, we see that, in the first place, both causes operate in the case of Mars, and secondly, that the submarine return currents from our polar regions must, at any rate, be due to the presence of ice in the polar seas. So that undoubtedly the Martial oceans, so far as their peculiar conformation will permit, are traversed by currents in various directions and at various depths.

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* If Herschel has completely overthrown Maury's theory that currents are altogether due to differences of specific gravity, saltness, and so on, Maury has at least been as successful in overthrowing Herschel's theory that the currents are due to the trade-winds. A theory more probable than either is, I think, that according to which the whole system of circulation is set in motion by the continual evaporation going on in equatorial seas. Thus, by a process resembling suction, an in-draught of cold water is caused, and this water coming from higher latitudes, where the earth's eastwardly motion is less, to lower latitudes, where the eastwardly motion is greater, produces the relatively cold and westwardly equatorial currents which exist in the Atlantic, Indian, and Pacific Oceans. Recent researches into the temperature of the deep sea have tended strongly to confirm these views.

Then, lastly, there must be rivers on Mars. The clouds which often hide from our view the larger part of a Martial continent, indicate a rainfall at least as considerable (in proportion) as that which we have on the earth. The water thus precipitated on the Martial continents can find its way no otherwise to the ocean than along river-courses.

As to the nature of these rivers again, we may form conjectures founded on trustworthy analogies. The mere existence of continents and oceans on Mars proves the action of forces of upheaval and of depression. There must be volcanic eruptions and earthquakes, modelling and remodelling the crust of Mars. Thus there must be mountains and hills, valleys and ravines, water-sheds and water-courses. All the various kinds of scenery which make our earth so beautiful have their representatives in the ruddy planet. The river courses to the ocean, by cataract and lake, here urging its way impetuously over rocks and boulders, there gliding with stately flow along its more level reaches. The rivulet speeds to the river, the brook to the rivulet, and from the mountain recesses burst forth the refreshing springs which are to feed the Martial brooklets.

Who can doubt what the lesson is that all these things are meant to teach us? So far, let it be remembered, we have been guided onward by no speculative fancies, but simply by sober reasoning. But can we pause just here? Shall we recognize in Mars all that makes our own world so well fitted to our wants — land and water, mountain and valley, cloud and sunshine, rain, and ice, and snow, rivers and lakes, ocean-currents and wind-currents, without believing further in the existence of those forms of life without which all these things would be wasted? Surely, if it is rashly speculative to say of this charming planet that it is the abode of life — if we must, indeed, limit ourselves to the consideration of what has been absolutely seen — it is yet to speculate ten thousand times more rashly to assert, in the face of so many probable arguments to the contrary, that Mars is a barren waste, either wholly untenanted by living creatures, or inhabited by beings belonging to the lowest orders of animated existence.

CHAPTER V.

JUPITER, THE GIANT OF THE SOLAR SYSTEM.

PASSING over the zone of asteroids, we come now to the noblest of all the planets — the giant Jupiter. If bulk is to be the measure of a planet's fitness to be the abode of living creatures, then must Jupiter be inhabited by the most favored races existing throughout the whole range of the solar system. Exceeding our earth some twelve hundred and thirty times in volume, and more than three hundred times in mass, this magnificent orb was rightly selected by Brewster as the crowning proof of the relative insignificance of the earth in the scale of creation — assuming only that we can indeed gauge the purposes of the Creator by the familiar tests of measure and weight.

Or if we estimate Jupiter rather by the forces inherent in his system, if we contemplate the enormous rapidity with which his vast bulk whirls round upon its axis, or trace the stately motion with which he sweeps onward on his orbit, or measure the influences by which he sways his noble family of satellites, we are equally impressed with the feeling that here we have the prince of all the planets, the orb which, of all others in the solar scheme, suggests to us conceptions of the noblest forms of life.

The very symmetry and perfection of the system which circles round Jupiter have led many to believe that he must be inhabited by races superior in intelligence to any which people our earth. The motions of these bodies afford indeed to our astronomers a noble subject of study. Our most eminent mathematicians have given many hours of study to the phenomena which the four moons present to the terrestrial observer. But we can trace only the general movements of the satellites of Jupiter. Their minor disturbances, the effects of the varying influences which the sun and Jupiter exert upon them, and which the moons exert upon each other, must tax the powers of far abler mathematicians even than he who "surpassed the whole human race in mental grasp."

But, after all, we must judge of Jupiter rather according to the evidence we have, and the analogies which are most directly applicable to the case, than according to fancies such as these. We know that the sun, which surpasses Jupiter in weight and volume even more than Jupiter surpasses the earth, is yet not the abode of life, so that mere size and mass must not be held to argue habitability. We know that many meteors and comets sweep through spaces more swiftly than the vast bulk of Jupiter, so that the energies indicated by mere velocity of motion, whether orbital or rotational, must be equally disregarded. Nor must we forget that, ages before men studied the motions of our own moon, she presented the same noble subject of study that she forms in our day for an Adams, a Leverrier, or a Delaunay. Even now a thousand grand problems are presented to our men of science which escape their notice; and we might as reasonably argue that there must be creatures existing unperceived among us, who deal with these problems, as that, out yonder in space, there must be beings who study the complicated motions of the Jovial satellites.

Jupiter presents the following principal physical habitudes:

He has a diameter of about eighty-five thousand miles, or nearly eleven times as large as the earth's, a surface one hundred and fifteen times larger, and, as I have said, a volume more than twelve hundred times larger. Gravity at his surface is about two and a half times as great as on our earth's, so that such creatures as exist around us would find their weight much more than doubled if they were removed to Jupiter. He lies more than five times farther from the sun than our earth, and the light and heat which he receives from that orb are reduced to about one-twenty-fifth of our supply. He rotates on his axis in rather less than ten hours (nine hours, fifty-five minutes, twenty-six seconds), so that the length of his day is considerably less than half of ours. His axis is nearly perpendicular to his orbit, so that there are no appreciable seasonal changes as he sweeps round the sun in his long year of 4,332½ days.

It will be convenient to consider, first, the probable influence of the great attractive power of Jupiter upon the dimensions of the various orders of living creatures existing upon his surface.

The grandeur of his orb naturally suggests, at first sight, the idea of beings far exceeding, both in might and bulk, those which live upon the earth. Old Wolfius was led to a similar conclusion in another way. I quote his quaint fancies as quaintly presented by Admiral Smyth. "Wolfius," says the genial sailor, "not only asserts that there are inhabitants in Jupiter, but also shows that they must necessarily be much larger than those of the earth; in fact, that they are of the giant kind, and nearly fourteen feet high by *eye*-measurement. And thus he proves it. It is shown in optics, that the pupil of the eye dilates and contracts according to the degree of light it encounters. Wherefore, since in Jupiter the sun's meridian height is much weaker than on the earth, the pupil will need to be much more dilatable in the Jovial creature than in the terrestrial one. But the pupil is observed to have a constant proportion to the ball of the eye, and the ball of the eye to the rest of the body; so that, in animals, the larger the pupil the larger the eye, and consequently the larger the body. Assuming that these conditions are unquestionable, he shows that Jupiter's distance from the sun, compared with the earth's, is as twenty-six to five; the intensity of the sun's light in Jupiter is to its intensity on the earth in a duplicate ratio five to twenty-six." The eyes of the Jovials and their dimensions generally must be correspondingly enlarged, and "it therefore follows that even Goliath of Gath would have cut but a sorry figure among the natives of Jupiter. That is, supposing the Philistine's altitude to be somewhere between eight feet and eleven, according as we lean to Bishop Cumberland's calculation, or the Vatican copy of the Septuagint. Now, Wolfius proves the size of the inhabitants of Jupiter to be the same as that of Og, king of Bashan, whose iron camp-bed was nine cubits in length and four in breadth — or rather he shows, in the way stated, the ordinary altitude of the Jovicolæ to be $13\frac{819}{1440}$ Paris feet, and the height of Og to have been $13\frac{1296}{1440}$ feet. See his Works, vol. iii., p. 438."

This exact determination of the dimensions of Jovial men would be very pleasing and satisfactory, were it not that another line of argument guides us at least as conclusively to a very different view. If we are to assume that beings resembling men in all attributes except size, actually exist on Jupiter, we might claim for these beings the power of moving from place to place as freely as we do, with quite as much reason as Wolfius claimed for them the same powers of vision that we possess. Proceeding according to this view, we are led to the conclusion that the *Jovicolæ* are pygmies about two and a half feet, on the average, in height. For we know that a man removed to Jupiter would weigh about two and a half times as much as he does on our own earth. He would thus be oppressed with a burden equivalent to half as much again as his own weight. This would render life itself an insupportable burden; and we have to inquire what difference of size would suffice to make a Joveman as active as our terrestrial men. Now, the weight of bodies similarly proportioned varies as the third power of the height; for example, a body twice as high as another — in other respects similar — will be eight times as heavy. But the muscular power of animals varies as the cross-section of corresponding muscles, or obviously as the square of the linear dimensions; so that of two animals similarly constituted, but one twice as high as the other, the larger would be four times the more powerful. He would weigh, however, eight times as much as the other. He would therefore be only half as active. Similarly, an animal three times as high as another of similar build, would be only one-third as active; and so on for all such relations. Now, since a terrestrial man removed to Jupiter would be two and a half times as heavy as on the earth, it follows obviously that a man on Jupiter proportioned like our terrestrial men would be as active as they are, if his height were to theirs as one to two and a half. Hence, setting six feet as the maximum ordinary height of men on the earth, we see that the tallest and handsomest of the Jovicolæ can be but two and a half feet in height, *if only our premises are correct*. Thus, Tom Thumb and other little fellows, if removed to Jupiter, might be wondered at for their enormous height, and eagerly sought after by any Carlylian Fredericks who may be forming grenadier corps out yonder.

One line of argument having thus led us to regard the Jovicolæ as Ogs of Bashan, while another equally plausible has reduced their dimensions to those of our two-year-old children, we may fairly conclude that this method of reasoning is fallacious. We must not measure the inhabitants of other worlds according to the conceptions suggested by the forms of life we are acquainted with upon earth. We must admit the possibility that arrangements, as different from those we are familiar with as the constitution of the insect is from that of man, may be presented amid the orbs which circle round the sun. It were unwise, no doubt, to give free scope to speculation where we have in truth no means of forming an opinion. We need not imagine, as some have done, that "the inhabitants of Jupiter are bat-winged," or with others, "that they are inveterate dancers." Nor, to take the views of more respectable authorities, need we agree with Sir Humphry Davy, that the bodies of the Jovials are composed of "numerous convolutions of tubes more analogous to the trunk of the elephant than any thing else;" with Whewell, that they are pulpy, gelatinous creatures, living in a dismal world of water and ice with a cindery nucleus; nor finally, with Brewster, that the Jovial may have his "home in subterranean cities warmed by central fires, or in crystal caves cooled by ocean-tides, or may float with the Nereids upon the deep, or mount upon wings as eagles, or rise upon the pinions of the dove, that he may flee away and be at rest." So soon as we give a definite form to the conceptions that the imagination, free from the control of exact knowledge, frames respecting the inhabitants of other worlds, we touch at once on the grotesque, the hideous, or the ridiculous.* It is sufficient to recognize the probability, or rather the certainty, that the beings of other worlds are very different from any we are acquainted with, without endeavoring to give shape and form to fancies that have no foundation in fact.

We may regard it as probable, however, that living creatures in Jupiter, if any exist, are built generally on a much smaller scale than those which people our earth. Trees, plants, and the vegetable world generally, must also, one would imagine, be very differently constituted from those we are familiar with. It is well known that the motion of the vegetable juices is in part regulated by the force of gravity, and therefore it must be admitted that the structure of terrestrial plants is in part dependent upon the value of gravitation at the earth's surface. Whewell, in his "Bridgwater Treatise" on the astronomical evidence of design in Creation, lays great stress on this relation, pointing out, if I remember right, that all vegetation would be destroyed at once if there could suddenly take place any marked change in the earth's attractive forces. If this view is correct, it is certain that none of our plants could thrive on the soil of Jupiter.

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* It may be worth while to gather a lesson from this circumstance. We know that every form of life is replete with evidences of adaptation (no matter how secured) to the conditions which surround it. We have thus evidenced to us, as forcibly as possible, the perfection of the laws by which the Creator rules the universe, and a measure (if one may so speak) even of that which is inconceivable by us — His infinite wisdom. Now, man, with all his knowledge of the Creator's ways, yet so soon as he passes the boundary of the known, pictures to himself all manner of unnatural and impossible forms of existence. Even the unknown parts of our own earth have been peopled ere now, in imagination, with "men whose heads do grow beneath their shoulders," and other similarly incongruous beings. It is more excusable perhaps, that an anatomically impossible structure should have been assigned to angels (the cherubim have been even more unfortunate), while the Evil One, that "goeth about as a roaring lion," has had the principal attributes of a class of *ruminantia* assigned to him.

The year of Jupiter differs in a much more striking manner than that of Mars from our terrestrial year. It consists of nearly twelve such years as ours, so that the period corresponding to one of our seasons lasts nearly three years, and a Jovial month is nearly equal to one of our terrestrial years. He has, however, no seasons in our sense of the word, since his equator is inclined but little more than three degrees to his orbit. Thus a perpetual spring reigns all over his surface.

But before we proceed to form a high opinion of the planet's condition under the influence of this perpetual spring, let us distinctly understand what the words mean. The word spring has a genial sound to ourselves, because we associate it with that which is commonly the pleasantest portion of our year; but it is just possible that the perpetual spring reigning over Jupiter, though doubtless well adapted to the wants of his inhabitants, leads to a state of things such as we might not find altogether so agreeable.

Admiral Smyth says that "as the rays of the sun fall perpendicularly on the body of the planet,* and always continue

* In the same paragraph Admiral Smyth says that, as seen from Jupiter's equatorial regions, the sun would seem to move through the heavens with great rapidity, while near the polar regions the sun's motion will be comparatively slow, and he will be seen to describe only a small semicircle above the horizon. The direct reverse is, however, the case, the sun's path and the rapidity of his apparent diurnal motion being nearly constant for all parts of Jupiter, and throughout his year. Admiral Smyth seems to have thought that the variations of the sun's path in Jupiter corresponded to those observed in the progress of a year at any place on the earth's equator, the sun always rising vertically

to do so, the heat must be as nearly as possible equal at all times of the year — a perennial summer: this is a striking display of beneficent arrangement." But we must be cautious in adopting this mode of argument in dealing with the Creator's ways. That the arrangement *is* beneficent, we need not of course question. But that we can recognize the way in which it is beneficent is quite another matter. If Jupiter's great distance from the sun is compensated for by this peculiar disposition of his axis, and we are to admire the beneficence thus displayed, are we therefore to find fault with the Creator for not dealing similarly with Saturn, Uranus, and Neptune, which, being farther from the sun, have greater need than Jupiter of some special adaptation of the sort? It seems safer to consider the consequences which flow from the arrangement without any special reference to the design of the Creator in permitting them, lest, in our over-anxiety to recognize beneficence in the treatment of one world, we should adopt a mode of reasoning which leads to the direct conclusion that other worlds have been ill-cared for.

The great peculiarity resulting from the arrangement in question — the only peculiarity, in fact, of which we can speak with any confidence — consists in this, that, everywhere on Jupiter, day and night are of equal length. It is in this sense only that perpetual spring — or perpetual autumn, if we please — reigns on the giant planet. The different latitudes of Jupiter have climates differing quite as much as those found in different latitudes on our own earth. At the equator the sun passes every day nearly to the point overhead. At the poles the sun seems to glide along the horizon, rising in the east, passing round — always near the horizon — toward the south, and thence to his setting-place in the west. In intermediate latitudes, the sun passes to a southerly elevation which is greater or less, according as the place is nearer to or farther from Jupiter's equator. It follows that there is a marked difference between the sub-equatorial and the sub-polar regions in Jupiter, while between these regions every intermediate climate is to be found.

and always describing a complete semicircle, though attaining different altitudes at different *seasons*. The real fact is, that in all parts of Jupiter the sun describes a complete diurnal semicircle, attaining a different midday altitude in different *places*. But, as he always rises nearly due east, and sets nearly due west (as he does in spring-time all over the earth), he necessarily crosses the horizon at different angles as seen in different places, and always describes about half of a great circle of the sphere.

Owing to the rapidity of Jupiter's rotation, the motion of the sun in the Jovial sky must be much more readily discernible and measurable than that with which the sun seems to pass across our own heavens. He traverses the whole semicircle, from the eastern to the western horizon, in two minutes less than five hours, or about six degrees in ten minutes. This corresponds to a motion through a space equal to the sun's diameter (as we see him) in fifty seconds, and must be readily discernible, even to the unaided vision of the Jovicolæ, unless their eyesight is much inferior to ours. The smallness of the sun, as seen from Jupiter, must help to render the motion more perceptible. He presents to them an apparent diameter only equal to about one-fifth of that with which we see him, so that in ten seconds he seems to pass over a space equal to his own diameter.

The other celestial bodies are affected with similar motions as seen from Jupiter. Of course, those seen near the poles of his heavens seem relatively at rest. One of these poles lis in the heart of the constellation Draco; the other lies close by the great Magellanic Cloud, which must present a magnificent cynosure to the inhabitants of the southern hemisphere of the planet. The contrast between the steadfastness of the polar star-groups and the swift motions of the equatorial constellations, must be impressive indeed. These equatorial groups are no other than our old friends the zodiacal constellations. As seen by the inhabitants of Jupiter, they rise with a perceptible but stately motion above the eastern horizon, pass to their culmination on the southern meridian, and so to their setting-place in the west — exhibiting the same splendors which the terrestrial astronomer delights to gaze upon, enhanced by the peculiar impressions of active power suggested by visible and obvious motion.

It may seem at first sight, that the presence of the Jovial satellites must tend to dim the splendor of the sidereal heavens. Our own moon, despite the beautiful passage* in which Homer has described the calm beauty of a moonlit night, certainly detracts largely from the magnificence of the star-groups; and as at times there must be four moons visible above the horizon of the Jovials, it might seem that all but the brighter stars would be quite obliterated. The first moon must appear somewhat larger than our own; the next has an apparent diameter rather more than half as large as that of our moon; the third (really the largest) appears about as large as the second; and the fourth has an apparent diameter equal to about a quarter of our moon's. Thus, in all, they cover a space on the sky more than half as large again as that which our moon covers. But, in reality, they cannot have nearly so marked an effect in dimming the lustre of the stars. For it must not be forgotten that they shine only by reflecting the sun's light, and that he illuminates them but faintly, in comparison with the light he pours upon our own moon. In effect, supposing their reflective capacities equal to the moon's, they must appear less brilliant than she does, in the proportion of about one to twenty-five; and combining this result with the above relation, it follows that, even if they could all be "full" together, they could send to the Jovials but about one-sixteenth part of the light we receive from the full moon. But, as a matter of fact, they cannot all be full together. The motions of the inner three are so related, that, though there is nothing to prevent them from being all visible together,* yet when so visible, one only can be full. The fourth may be full at the same time, or, in fact, may be combined with the other three in any way, since its motions are not bound up with theirs as theirs are *inter se.*

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* Homer must not be held responsible for Pope's amazing description, which, strangely enough, has found an ardent admirer in one of our best modern observers. Homer did, however, mention as a characteristic of the moonlit sky, that "all the stars shine," a proof that some times, as Horace tells us, the great master nodded.

Even now, however, we have not reached a full estimate of the extent of the mistake which those astronomers have made who speak of the splendor with which the satellites of Jupiter illuminate his skies. When at that part of their orbits where they would otherwise be full, the three inner moons are always eclipsed, and though the fourth, by reason of its great distance,† sometimes escapes eclipse, yet more frequently it is obscured like the others. The two inner satellites are eclipsed for upward of two hours, and as they occupy but a few hours in completing their circuit round the sky,* it will be seen how largely this relation detracts from their light-supplying powers.

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*Or all invisible together. Lardner asserts the contrary: so that one would imagine he had never seen all the moons together on the same side of Jupiter.

† Not on account of the inclination of its orbit being large, as Sir John Herschel has said. The orbit of this satellite is, indeed, less inclined than the orbits of the others.

We see, then, that those writers have been mistaken who allege that the great distance of Jupiter from the sun is compensated by the number of the moons, and the quantity of light they reflect toward him. So far is this from being the case, that, under the most favorable circumstances, they can supply during the Jovial night but about one-twentieth part of the light with which the full moon illuminates our nocturnal skies. The poetical descriptions which imaginative writers have indulged in, respecting the splendor of the scene presented by these satellites, will not bear the dry light of numerical estimation. That the satellite-system of Jupiter subserves important functions, and affords, in reality, like all the works of the Creator, the amplest evidence of design, need not be questioned; but that we have been able to understand the special purpose for which they have been created — in fine, "to see," as the Creator does, "that they are good"— may be assuredly denied.

Perhaps, if one were able to discuss with advantage the special purposes which this or that portion of creation is intended to subserve, it might be argued that the outer planets have greater need of moons than the inner, because, their year being longer, there is greater occasion for objects whose motions shall serve as measures of time. The satellites of Jupiter supply, by their separate motions, convenient measures of the shorter time-intervals; while, by their successive conjunctions, (i.) in pairs, (ii.) the three inner together, and (iii.) the outer with pairs of the inner, they afford convenient measures of longer intervals.

But let us turn from vague guesses at the purposes of the Almighty, to the consideration of those facts which are actually presented to our notice.

Recognizing the existence of varied climatic relations in different parts of Jupiter, we have now to consider the climate of the planet generally, to contemplate the position of this great orb in the solar system, and to determine how far its great distance from the sun may be compensated by other relations.

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* Moving in a direction contrary to that due to the rotation of Jupiter, they of course remain longer above the horizon than the sun, or the equatorial fixed stars.

There can be no doubt that the amount of heat poured by the sun on any portion of Jupiter's surface, placed perpendicularly with respect to the heat-rays, must be very much less than the amount received by an equal portion of our earth's surface, similarly situated. The direct heating effects of the sun must, in fact, as already stated, be less on Jupiter than on our own earth, in the proportion of about one to twenty-five. And it cannot be doubted that the effects of this difference must be highly important, whatever arrangements may exist to compensate for the deficiency of heat. If we can demonstrate in any way that the mean temperature of the Jovial atmosphere is equal to that of our own air, or even greater, yet the difference of the sun's direct heat involves a variety of consequences which we cannot disregard.

We know, for instance, that it is principally the direct heat of the sun that causes the evaporation of water from the surface of oceans, seas, lakes, and rivers, and therefore all the important consequences which flow from the presence of aqueous vapor in large quantities in the earth's atmosphere. We can conceive the existence of vapors in the air which might keep away from the earth's surface the greater portion of the sun's heat, and yet, by preventing the escape of the remainder by radiation into space, might leave the general warmth of the air around us as great as it is at present. But it cannot be doubted that such an arrangement would injuriously affect the whole economy of evaporation and its consequences, winds, rains, clouds, mist, with *their* consequences, so important for the welfare of terrestrial races.

And in like manner other effects accruing from the direct action of the solar rays might be considered.

It follows, then, that it is by no means sufficient to show how the heat which falls upon Jupiter may be stored up, through the action of some component of his atmosphere in preventing its radiation into space. It is, indeed, of the utmost importance to know that even this is possible, because we are thus enabled to see that Jupiter is not necessarily an abode so bleak and desolate as some writers have imagined. In the following passage, Prof. Tyndall has exhibited the means by which this result may be brought about, and the inhabitants of the noblest planet in the solar system placed somewhat higher in the scale of creation than Whewell surmised. "In these calculations," he remarked, referring to Whewell's estimate of the sun's heating power on Jupiter and the other exterior planets, "the influence of an atmospheric envelope was overlooked, and this omission vitiated the entire argument. It is perfectly possible to find an atmosphere which would act the part of a *barb* to the solar rays, permitting their entrance toward the planet, but preventing their withdrawal. For example, a layer of air, two inches in thickness, and saturated with the vapor of sulphuric ether, would offer very little resistance to the passage of the ether rays, but I find that it would cut off fully thirty-five per cent. of the planetary radiation. It would require no inordinate thickening of the layer of vapor to double this absorption; and it is perfectly evident that, with a protecting envelope of this kind permitting the heat to enter but preventing its escape, a comfortable temperature might be obtained on the surface of our most distant planet." The difference between such an arrangement as this and the way in which the earth's temperature is obtained, is the exact converse of that dealt with when we were considering the case of Mercury and Venus. Precisely as the mean temperature of the atmosphere of either of the interior planets may be no higher than that of our own air, while yet the sun's direct rays continue wholly unbearable, so the outer planets may have a perfectly comfortable temperature, while yet that direct solar heat which exerts so many important influences on the earth must be supplied only in quantities which we should find wholly inadequate for our wants.

I am far from desiring to infer that Jupiter must therefore be uninhabited, or even that the creatures existing on his surface must necessarily differ wholly in their nature from any with which we are familiar. But I think that, while, on the one hand, we must reject one of the chief arguments by which Whewell was led to people Jupiter with cartilaginous and glutinous creatures (!) floating in boundless oceans, so, on the other, we cannot accept without question the argument by which an effort has been made to indicate the possibility of a close correspondence between Jupiter's climate and our earth's.

And here we are led to the most interesting and suggestive of all the relations exhibited by Jupiter, or rather to three closely-associated relations, which lead to views of a somewhat startling character.

In common with the other large planets lying outside the zone of the asteroids, Jupiter has a mean density falling very far short of the mean density of the earth or the other small planets which travel within that zone. According to the best estimates of his mass and apparent diameter, his mean density would seem to be rather less than one-fourth of the earth's, or greater than the density of water by about one-third. It is worthy of remark, in fact, that his density is almost exactly the same as the sun's, and considerably greater than that of the other three outer planets hitherto discovered.

If we were quite certain that the disk measured by us exhibits the real outline of the planet, or that his atmosphere was not of abnormal extent, and that his globe was solid throughout, it would follow that the substances composing Jupiter were either altogether different from those forming our earth, or that they were combined in very different proportions. On the last point we can form no opinion. On the first we must be guided by the appearance of the planet.

Thus we are led to the second of the three relations just mentioned — the appearance of well-marked but variable belts on the planet — and of other indications implying the existence of an atmosphere of great extent.

The belts of Jupiter are commonly arranged with a certain symmetry on either side of the great equatorial bright belt, but sometimes there is a rather marked contrast between the northern and the southern halves of the planet. In color the dark belts are usually — when seen with suitable telescopic power* — of a coppery, ruddy, or even purplish tint, while the intermediate light bands vary from a pearly white in the equatorial belt, through yellowish white in the middle latitudes of both hemispheres, to a grayish or even bluish tint at the poles. The picture of Jupiter which forms the frontispiece, while exhibiting many of the features usually seen, is intended specially to illustrate relations presently to be dealt with.

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* What is required is not so much a high light-gathering as a high magnifying power, though both points are of importance. When the light is not adequately reduced by increase of magnifying power, the color is lost in the resulting "glare." Reflectors seem to have an advantage over refractors in exhibiting the colors of the planets; at least, nearly all the accounts in which the appearance of color has been specially dwelt upon, have been received from observers who have used reflectors.

There is every reason to believe that these belts indicate the existence of a very extensive vapor-laden atmosphere. The dark belts must not be considered as the true cloud-belts, because it must be remembered that we look upon the reverse side of the skyscape presented during the day to the Jovials: so that where they see densely-compacted dark clouds, we see the light which those clouds have intercepted; and, on the other hand, where they see clear spaces, the light which reaches them is not reflected to us without a considerable loss of brilliancy. Thus the dark belts of Jupiter are those regions where — if at all — we see the true surface of the planet.

Now, viewing the belts in this light, have we any means of judging from their aspect what is the extent of the planet's atmosphere? So far as I know, the question has never been considered, but it is well worthy of careful study.

It seems clear, in the first place, that if the bright belts really are cloud-belts, and the dark belts the surface of the planet, then on the edge of the planet's disk we ought to see some irregularity of level — the cloud-belts projecting slightly beyond the real outline of the planet — if the atmosphere have that enormous extent which some astronomers have supposed. Whether such an appearance has ever been looked for I do not know, but it has certainly never yet been detected.

We are forced to conclude, then, that either the atmosphere of Jupiter is not sufficiently extensive to interfere appreciably with our measurement of the planet's bulk, or else the dark belts belong but to a lower cloud-layer, not to the planet's real surface.

We have further evidence on this point in the appearance of dark spots on the dusky belts. These spots have even been described as black, though surely their appearing of that hue must be ascribed to the effect of contrast. Now, these dark spots, which have been seen by Cassini, Mädler, Schwabe, Airy, and others, may be regarded as the real surface of the planet (unless they belong to a yet deeper cloud-layer), seen for a while through openings in the cloud-bed to which the dusky belts belong. The reader will not fail to notice here some resemblance to what has been already mentioned respecting the sun-spots; and when we come to the third and most striking of the associated features I am now dealing with, it will be seen that there may be more in the analogy than one might at first sight be disposed to imagine.

How far the appearance of small round white spots on the dark belts may be considered as indicative of the extent and constitution of the Jovial atmosphere, it is not very easy to determine. That they are dense clouds, hanging suspended above the dusky cloud-layer, must be admitted as highly probable, but it is open to question whether they have formed there in the same way that cirrus-clouds are seen to form at a great elevation above a layer of cumulus clouds, or whether they indicate the action of volcanoes beneath the dusky layer, propelling enormous streams of vapor through the superincumbent cloud-beds.

The third point on which I have to dwell is the variability of the belt-system, under which head I include not only variations in shape and extent, but those much more significant changes of color which have been recently discovered.

So far as is yet known, there is no recognizable law in the changes of shape exhibited by the belts of Jupiter — no periodicity or intelligible sequence. It may be suggested, in passing, that a systematic and persistent scrutiny of the planet might lead to the discovery of laws of this sort, which could not fail to indicate physical conclusions of the utmost importance. Nay, further, since we cannot doubt that the condition of the real surface of Jupiter is in some sort reflected, so to speak, in the aspect of his cloud-envelopes, it seems far from unlikely that a scrutiny of this sort might tell us where his oceans and continents, where his deserts, lakes, or rivers, are situated, even though no direct evidence of their existence might ever reward the observer. In these days, however, nine-tenths of those who are fortunate enough to possess fine telescopes prefer either to leave them idle, or to employ their powers in making observations, at great pains and labor, which are not worth the paper on which they are recorded.* The few original observers we have are overtasked by the multitude of questions of interest presented to their consideration, so that many subjects of inquiry must perforce wait, either till their turn arrives, or till those who have the means of studying them choose to turn their thoughts from the sterile subjects they are now engaged upon.

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* It is painful to those who know what might be done in the numerous fine observatories now existing throughout England, to see the powers of many noble instruments — the *chef-d'œuvres* of English and Continental opticians — devoted to puny imitations of the work done at Greenwich and other similar establishments. I speak on the authority of one of the first, if not the very first, of our professional astronomers, when I say that these imitations, even though they approached in char-

So far, then, as inquiries have as yet been pushed, all that can be asserted on the subject we are considering is, that the planet's belts vary greatly in form, extent, and general appearance. At one time the dusky belts cover a large proportion of the planet's disk, at another they are singularly narrow. Now they are very regularly disposed, now they seem in some way under the action of disturbing forces of great intensity, causing

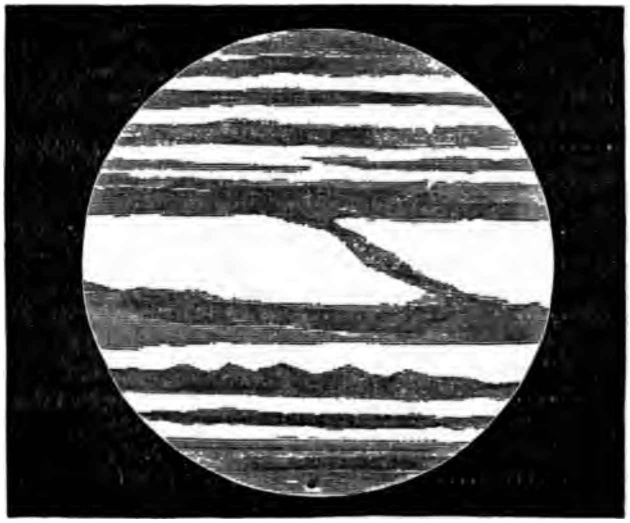

FIG. 1.—The Planet Jupiter (Browning).

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acter — which they do not and cannot — the operations superintended so ably by the Astronomer Royal, would be a simple waste of time and labor. Nor is this the only way in which fine telescopes are wasted. While on every side there are subjects of research which most pressingly require investigation, many of those who possess the requisite means and leisure for the purpose — nay, are not wanting in the necessary taste for observational research — are unhappily applying themselves to going over, perhaps with relatively inferior powers, ground which has already been thoroughly ransacked by our great observers. With some ten or twelve exceptions — which it is unnecessary to name — our private observatories seem to have banished every thing resembling originality.

them to assume the most irregular figure. The foregoing picture of the planet (Fig. 1) as seen by Mr. Browning, with one of his own reflectors, indicates an appearance not uncommonly seen, a dark streak extending obliquely across the planet's equatorial regions. The number of belts is singularly variable. Sometimes only one has been seen, at others there have been as many as five or six on each side of the planet's equator. In the course of a single hour, Cassini saw a complete new belt form on the planet, and on December 13, 1690, two well-marked belts vanished completely, while a third had almost disappeared in the same short interval of time.

But if we seem to recognize here the action of forces much more intense than those which influence the condition of the earth's atmosphere, we have still more striking evidence to the same purpose in the changes of color which have recently been detected in the great equatorial belt. This belt is usually of a pearly white tint, and has long been recognized as one of the most constant features of the planet's aspect. As the mean surface of this belt cannot be less than a fifth of the whole surface of the planet, it is clear that any changes which may take place in its general aspect cannot but be of the utmost significance. Now, during the autumn of 1869 and the spring of 1870, this belt has been more strongly colored than any part of the planet. Mr. Browning (to whom I am indebted for the beautiful painting of Jupiter, which formed the design from which the frontispiece has been taken), observing Jupiter in the earlier part of the above-named interval, found the equatorial belt of a greenish-yellow color, which deepened in October, 1869, to a full ochreish yellow, and in January of the present year had assumed an even darker tint, resembling yellow ochre. On one occasion, and on one only, he detected this tint in the first bright belt north of the equator. While thus exhibiting strongly-marked and changing colors, the equatorial belt has lost its right to be called, *par excellence,* the bright belt of the planet, being considerably inferior in brilliancy to the narrow bright belts north and south of it.

Other observers have also seen these colors. Mr. Slack, with a 6-inch Browning-With reflector, and Mr. Brindley, with an 8½-inch telescope of the same construction, have witnessed most of the changes of color above described; and I myself, using

Mr. Browning's 12¼-inch telescope, found the greenish-yellow tint of the equatorial belt last autumn altogether unmistakable.*

In the phenomena here described we have a problem whose interpretation is far from easy. Changes in the shape, disposition, and extent of the dark belts are sufficiently intelligible when we associate them, as we seem justified in doing, with variations in the position of the currents which traverse the vaporous envelope of Jupiter as the trades and counter-trades traverse the earth's atmosphere. But the equatorial zone is Juipter's belt of calms, resembling in this respect the equatorial region, called by sailors the "doldrums," and, though occasional storms might be expected to agitate this region, yet processes of change, continuing for several months in succession, can evidently not be attributed to any such cause. We are taught, by the progress of recent research, to regard the color of the light derived from any source as a relation of the most instructive character, and changes of color, especially changes affecting so enormous a body as Jupiter, and so extensive a proportion of his surface, cannot but be looked upon as highly significant. Supposing we regard the ordinarily white light of the equatorial belt as indicative of the existence of enormous masses of cloud reflecting ordinary solar light to us, then we should have to regard the appearance of any other color over this region as an indication that these cloud-masses had been, through some unknown cause, either wholly or in part swept away. But — passing over the objection that this view leaves our difficulty unexplained — even if we assumed that in this way a portion of the surface of Jupiter had been brought into view, wholly or partially, why should this surface not exhibit a constant appearance? We cannot suppose changes affecting Jupiter's real surface are taking place with sufficient rapidity to explain the series of strange color changes observed by Messrs. Browning, Slack, and other astronomers. But if, on the other hand, we assume that a portion of the light ordinarily received from the bright belt is inherent — that is, that the planet is, to some extent, self-luminous — then there remains the difficulty of explaining by what conceivable processes the equatorial regions are filled with a yellow light, so full and bright as to reach our earth from beyond four hundred millions of miles.

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* I had written thus far only, when I attended the meeting of the Royal Astronomical Society on January 14, 1870, where Mr. Buckingham, the owner of the great refractor, 21¼ inches in aperture, from whose performance so much was expected, mentioned that, as seen with this powerful instrument, the great belt was resolved into a number of small colored clouds on a white ground.

But I have spoken of the three relations last considered — the small density of Jupiter, his extensive atmosphere, and the changes which take place in the shape and color of his belts — as associated phenomena. It remains that I should endeavor to justify this statement.

We know that Whewell, reasoning from the low specific gravity of Jupiter, was led to the conclusion that either the substance of the planet is wholly watery, or else a few cinders in the centre of Jupiter's globe constitute the only solid portion of his substance. It need hardly be said that the whole progress of modern astronomy is opposed to this view. We have seen that in the sun the same elements exist as in the earth, and that in the only planet whose nature we have been able to examine satisfactorily we find evidence of the existence of the same forms of matter that we see around us. It cannot but be held as highly improbable that the earth is the only member of the planetary system whose substance thus closely resembles that of the parent orb, nor is it likely that Mars is the only planet whose general atmospheric constitution resembles the earth's. Far more probably the lesson we are really to learn from these circumstances is, that throughout the solar system a general similarity of constitution exists, the sun being, so to speak, the type of the family over which he rules. Differences of *condition* we are compelled to recognize, since the sun itself, though constituted of the same elements as the earth, is in so different a state and has a mean density relatively so small; but we have no evidence justifying us in believing that any important differences of constitution exist throughout the solar system.

Thus, we are led to regard the singularly small density of Jupiter and of the other planets outside the orbits of the asteroids, as due rather to some peculiarity in the condition of these orbs than to any such peculiarity of structure as Whewell insisted on. It will be seen at once that Jupiter's extensive atmospheric envelope and the strange changes in the aspect of his belts are circumstances which tend strikingly to confirm this impression. Let it be remembered that, supposing Jupiter's globe even to be wholly covered with water, yet a sun twenty-five times farther off than ours could not by any possibility load his atmosphere with the enormous masses of vapor actually present in it. Let it be remembered, further, that the relatively sluggish action of the sun upon Jupiter could not by any possibility give rise to atmospheric disturbances so tremendous as those which are evidenced by the rapid changes of figure of his cloud-bands.[*] When to this we add the relative minuteness of the seasonal changes on Jupiter, we see at once that, unless some other cause than solar action were at work, the condition of Jupiter's atmosphere ought to be very much calmer than that of the earth's.

It seems to me that these considerations point with tolerable clearness to the conclusion that, within the orb which presents so glorious an aspect upon our skies, processes of disturbance must be at work wholly different from any taking place on our own earth. That enormous atmospheric envelope is loaded with vaporous masses by some influence exerted from beneath its level. Those disturbances which take place so frequently and so rapidly are the evidences of the action of forces enormously exceeding those which the sun can by any possibility exert upon so distant a globe. And if analogy is to be our guide, and we are to judge of the condition of Jupiter according to what we know or guess of the past condition of the earth and the present condition of the sun, we seem led to the conclusion that Jupiter is still a glowing mass, fluid probably throughout, still bubbling and seething with the intensity of the primeval fires, sending up continually enormous masses of cloud, to be gathered into bands under the influence of the swift rotation of the giant planet. No otherwise, as it seems to me, can one explain the intense vitality, if one may use the expression, of a planet circumstanced as Jupiter is. No otherwise can one understand whence his atmosphere is loaded with vapor-masses whose contents must exceed, on a moderate computation, all the oceans on the surface of this earth. When we see masses so enormous swayed by influences of such energy, that intermediate belts, thousands of miles in width, are closed up in a single hour;* when we recognize the tremendous character of the motions which, from beyond four hundred millions of miles, are distinctly cognizable by our telescopes, we see that we have no ordinary phenomena to deal with, and that the theory we adopt for their explanation cannot be otherwise than striking and surprising.

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[*] It is worthy of consideration, also, that even though the sun acted as efficiently upon the air and oceans of Jupiter (assumed to be similar to our own), yet atmospheric disturbances (due chiefly, as we know, to these two forms of action) could not possibly be so violent even as on our own earth, since corresponding latitudes of Jupiter (that is, regions where corresponding effects would be experienced) are separated by distances so very much greater. It is clear that, if along a certain zone of a planet the sun exerts a certain amount of influence, while along another he exerts a different influence, the result of the difference, looked on as a cause of atmospheric disturbance, must be smaller as the distance between the zones is greater.

If the view which I have here put forward — or rather, the view to which I have been led by a careful consideration of the phenomena which Jupiter presents to our contemplation — be indeed correct, we must of course dismiss the idea that the giant planet is at present a fit abode for living creatures. Yet need we not turn from his system with the thought that here at least our hopes of recognizing other worlds have been disappointed. If Jupiter be still in a sense a sun, not indeed resplendent like the great centre of the planetary scheme, but still a source of heat, is there not excellent reason for believing that the system which circles around him consists of four worlds where life — even such forms of life as we are familiar with — may still exist? Those four orbs, which our telescopes reveal to us as tiny points of light, are in reality globes which may be compared with the four worlds that circle nearest to the sun. I have shown that they cannot subserve the purpose which many astronomers have ascribed to them, of compensating Jupiter for the small amount of light he receives, even if they could be seen from any point of his cloud-encompassed surface. So that, even adopting the commonplace and superficial view that the purpose of any object may be regarded as ascertained when we have been able to ask (without any obvious answer) what other purpose it *can* subserve, we still are led to the belief that the satellites of Jupiter must be the abode of life, since on this view, and on this view only, we find a *raison d'être* both for the planet and for the system which circles round him.

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* Even if we take the disappearance of a dark belt to be due to the formation of clouds, which is perhaps more probable than that the clouds of neighboring belts have *closed in*, the forces represented by the change are nevertheless tremendous.

There are no considerations which appear directly opposed to the view that Jupiter is in a sense a sun. It need hardly be said that I do not regard him as being in the same condition as the central luminary of the planetary system. He is not an incandescent body, or, if he is, the greater part of his light is veiled by the cloud-envelopes which surround him. The solar clouds are, as we know, themselves luminous; those of Jupiter are not so, a circumstance which indicates that the heat of Jupiter is not sufficient to vaporize those substances which are incandescent when in the liquid state. The outer layer of clouds must, therefore, be regarded as for the most part aqueous. We see *there,* in fact, the future oceans of Jupiter, if the hypothesis I am now dealing with be correct.

That Jupiter may supply an immense amount of heat to his satellites (on this view of his condition) is perfectly clear, since the amount of light he emits is no adequate measure of the amount of obscure heat which radiates from him to the four worlds around him. When we consider the enormous apparent size of Jupiter as seen from his satellites, we recognize at once how large a supply of heat he is capable of transmitting to them. From the outermost satellite his apparent diameter exceeds that of the sun (as seen by us) some eightfold, and his apparent size, therefore, exceeds the sun's more than sixtyfold. From the innermost he is seen with a diameter nearly forty times that of the sun, and with an apparent area more than fourteen hundred times as large as his.

We have evidence, however, which renders it far from improbable that Jupiter may emit some small proportion of light. I have already referred to the singular excess of his brilliancy over that due to his size and his distance from the sun and from us. The estimates of Zöllner, the eminent photometrician, serve to show, not, indeed, that Jupiter sends more light to us than he receives from the sun, but that he sends much more light than a planet of equal size and constituted like Mars, the moon, or the earth, could possibly reflect to us if placed where Jupiter is. Whereas Mars reflects but one-fourth of the light he receives, Jupiter reflects more than three-fifths. The moon sends less than a fifth; Saturn, Jupiter's brother giant, more than a half. The late Prof. G. Bond, of America, actually calculated that Jupiter sends forth more light than he receives. Whether his observations or the more systematic observations of the German astronomei are accepted, we see that, unless we adopt some such hypothesis as I have dealt with above, we must recognize a marked difference between the relative light-reflecting capacities of the two largest planets of the system, and those of Mars or the moon. In fact, from other researches of Dr. Zöllner's it follows that, if Jupiter do not shine in part by native light, his surface must possess reflective powers nearly equal to those of white paper. Now, this would scarcely be credible, even though under the telescope the planet's surface were found to be universally white; but, as we find a large proportion of it to be of a dull coppery hue, we seem forced to admit that it cannot really have an average reflective power nearly so great as that calculated by Zöllner. It follows, as at least highly probable, that Jupiter shines in part by his own light, and this being admitted, we cannot but regard it as highly probable that the mass of the planet must be intensely hot.

It may seem, at first sight, that the apparent blackness of the satellites' shadows, as seen on the disk of Jupiter, is wholly opposed to the view that any portion of his light is native. But, as a matter of fact, there is no force at all in this consideration, or rather, whatever weight we may attach to the observed appearance of the satellites' shadows is in favor of the strange theory here put forward. For it has been a subject of remark among the most experienced observers, that a satellite in transit will occasionally appear as dark as its shadow, both seeming black. The blackness, then, is only apparent, and an effect of contrast. In reality, if such observations as I have mentioned are to be trusted (and I know no reason for disregarding them), the shadow of a satellite is not black, and therefore there seems no escape from the conclusion that the surface on which they are projected is partially self-luminous.

A stronger argument against the belief that Jupiter is self-luminous, lies in the fact that the satellites disappear in his shadow. It must be remembered, however, that in any case we can assign but a small proportion of inherent light to Jupiter, and that his satellites would, therefore, in any case, lose so large a proportion of their light when passing into his shadow, that we might expect them to disappear, even under the closest telescopic scrutiny.

Although I have already far exceeded the limits I had proposed to myself for the consideration of this noble planet, it is with regret that I take leave of him to pass onward to the outermost bounds of the solar system. I would fain dwell even longer than I have, on a subject of contemplation at once so interesting and so instructive. Jupiter, the centre of a noble system of worlds, or Jupiter, himself a world, inhabited by beings as high perhaps in the scale of creation as he himself is in the scheme of the planets, is alike a worthy subject of study. The more one dwells on the features he presents, the more one is impressed with the sense of the grandeur of his position in the universe. Surely, whether now inhabited or not, he *must* be intended to be one day the abode of noble races. Surely no astronomer worthy the name can regard this grand orb as the cinder-centred globe of watery matter so contemptuously dealt with by one who, be it remembered thankfully, was not an astronomer. He who has not gazed hour after hour on the glories of the giant planet, gathering fresh delight as feature after feature is revealed beneath his scrutiny — he who takes his astronomy but at second-hand from the pages of the real worker, turning from labors in other fields "to see what these star-gazers have to say," may lightly disregard the grand lesson which the heavens are always teaching, and find only the grotesque and the incongruous, where in reality there is the perfectest handiwork of the Creator. But the astronomer, imbued with the sense of beauty and perfection which each fresh hour of world-study instils more deeply into his soul, reads a nobler lesson in the skies. The music which reaches his ears may be fitful, but it is not "as sweet bells jangled out of tune and harsh"; he may not master its full meaning, though every note thrills through his inmost soul; but, even when its sounds are least distinct, they have a beauty and solemnity which are all their own. In fine, the true astronomer may say with the Pythagorean, but in another sense:

> "There's not one orb which thou behold'st
> But in his motion like an angel sings,
> Still quiring to the young-eyed cherubim;
> But while this muddy vesture of decay
> Doth grossly close us in we cannot hear it."

CHAPTER VI.

SATURN: THE RINGED WORLD.

IF JUPITER by his commanding proportions affords a forceful argument against the view that our tiny earth is the only real world in the solar system, Saturn supplies an argument of scarcely inferior strength in the singularly complex character of the scheme of which he is the centre. No one can contemplate this glorious planet, as shown by a telescope of adequate power, without being impressed by the conviction that he is looking at a world altogether more important in the scheme of creation than the globe on which he lives. Whether he recognizes in the present condition of the planet the result of the action of those laws which the Almighty has assigned to His universe, or whether he prefers the view that Saturn and his system are seen now as they were fashioned at the beginning by the Almighty's creative hand, he is alike amazed at the wealth of design exhibited in the scene he is gazing upon. He may not be able, indeed, to appreciate the true character of the purposes which the various parts of the Saturnian system are intended to subserve, or he may, in the rash attempt to solve the mighty problem, be led to erroneous conceptions; but that the great planet *is* designed for purposes of the noblest sort, he cannot gravely question.*

In volume and mass Saturn is inferior to Jupiter. Jupiter is twelve hundred and thirty times, Saturn is not quite seven hundred times as large as the earth; and, while Jupiter outweighs her three hundred times, Saturn is scarcely ninety times as heavy as she is. Still Saturn is sufficiently large and massive to dwarf our earth to insignificance; and even Uranus and

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* I know nothing better calculated to lead men to choose astronomy as their favorite subject of study, than the contemplation of the Saturnian system. I can well remember the sensations with which I saw the ringed planet for the first time. I look on that view as my introduction to the most fascinating of all the sciences.

SATURN

[illegible]

Neptune, though belonging to the family of the major planets, and giants compared with the earth, fall below Saturn far more than he does below Jupiter.

Like Jupiter, Saturn rotates very rapidly on his axis, the length of his day being about 10½ of our hours. The materials of which Saturn is composed have a mean density not much greater than half that of Jupiter, or less than three-fourths of the mean density of water. In fact, Saturn's substance is specifically lighter than that of any known planet. It seems not impossible that we have in this relation some indication of the true cause of that complexity of detail which the Saturnian system exhibits.

The equator of Saturn is inclined about 28⅕ degrees to the plane in which the planet moves, so that his seasons (so far as they depend on this circumstance) closely resemble in character those of the planet Mars. He occupies about 29½ years in circling once round the sun — this therefore is the length of the Saturnian year. His distance from the sun is nearly twice that of Jupiter, and nearly ten times that of the earth; so that the amount of light and heat which any portion of his surface receives from the sun is about $\frac{1}{91}$st part of that received by a similar portion of the earth's. His orbit being somewhat eccentric, however, there is a considerable variation in this respect during the course of a Saturnian year, insomuch that when he is nearest to the sun he receives more light than when in aphelion in the proportion of about five to four.

Most of the relations which have to be considered in discussing the habitability of Saturn have been already dealt with (under very similar conditions) in treating of other planets; so that I propose to touch on them very lightly, in order to come more quickly to those circumstances which distinguish Saturn specially among the other members of the solar system.

Gravity at his equator is almost exactly equal to gravity at the earth's surface. Near the poles there is a marked increase in the action of Saturnian gravity, insomuch that a body weighing ten pounds at his equator would weigh about twelve pounds at either pole. There is nothing, however, in this peculiarity which need be specially dwelt upon.

The length of the Saturnian year, and the small quantity of light and heat received from the sun, are simply more marked instances of what has already been considered in the case of Jupiter. We may conclude with some confidence that these relations are quite sufficient to render Saturn wholly uninhabitable by such creatures as exist upon the earth; but there seems no reason for supposing that (so far as these relations alone are concerned) the planet may not be the abode of living beings as high in the scale of creation as any which live upon our globe.

And thus viewing Saturn, we cannot regard even the exceptional effects produced by his ring-system as of themselves sufficient to banish life from his surface. These effects are not without interest, however, and, as they have been made the subject of some discussion, I may be permitted to make a few remarks upon them.

I apprehend that, when Sir John Herschel said that the rings occasion an eclipse of nearly fifteen years in duration, first to the northern and then to the southern hemisphere of the planet, he meant simply that during an interval of such length a large portion of either hemisphere was in shadow. He knew perfectly well that, long after the edge of the ring has been turned directly toward the sun, a very large proportion of the hemisphere, over which the ring's shadow proceeds to sweep, remains illuminated. It had always seemed to me, therefore, altogether a mistake on the part of Dr. Lardner to interpret Herschel's words as though implying that a whole hemisphere of the planet is eclipsed for fifteen years in succession.

So misinterpreting the expression used by Sir John Herschel, Dr. Lardner, in his desire to show that no such relation existed, was led into real mistakes which a sounder mathematician would not have fallen into. He examined the relations presented by the ring in a *quasi*-mathematical, but inexact manner, and came to the following conclusions: "That, by the apparent motions of the heavens produced by the diurnal rotation of Saturn, the celestial objects, including the sun and the eight satellites, are not carried parallel to the edges of the rings; that they are moved so as to pass alternately from side to side of these edges; that, in general, such objects as pass under the rings are only occulted by them for short intervals before and after their meridional culmination (*sic*); that, although, under some rare and exceptional circumstances and conditions, certain objects

— the sun being among the number — are occulted from rising to setting, the endurance of these phenomena is not such as has been supposed, and the places of their occurrence are far more limited." All these statements are more or less incorrect, and most of them are the direct reverse of the truth. The seven inner satellites of Saturn stand in an altogether different relation, with respect to the rings, than all other celestial objects, since they travel in the same plane and in circles concentric with the outlines of the rings: they can, therefore, no more be occulted by the rings, than an outer ring can be occulted by an inner one. So far is it again from being true that the sun is in general only occulted for a short time before and after culmination, that the more common case (considering the whole planet) is for the sun to be eclipsed (if at all) throughout the whole of the Saturnian day; and a very common case, left altogether unnoticed by Dr. Lardner, is, that the sun is occulted in the forenoon and afternoon, but free from eclipse in the middle of the day. Nor is it true that the places where the sun can be totally eclipsed throughout the day are limited to a relatively small portion of the planet, since every part of the planet whence the rings are visible at all has the sun eclipsed by the rings throughout the whole day for a longer or shorter succession of rotations, and, in the remaining or polar regions of the planet, the sun is altogether absent for long intervals of time, for the same reason that he is absent from the skies of our polar regions during a comparatively short interval. As for the endurance of the total diurnal eclipses, it is only necessary to remark that, in Saturnian latitudes corresponding to that of London or Paris, the sun is totally eclipsed for more than five years in succession, while in a latitude corresponding to that of Madrid he is totally eclipsed for nearly seven years in succession. This suffices to show that an arrangement which the inhabitants of earth would find wholly unendurable prevails over a very large proportion of Saturn's surface.*

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* The views here expressed as to the effects of the Saturnian rings are founded on exact mathematical calculation, of which the elements are given in my treatise on Saturn. The problem is not by any means a difficult one, and the only way in which the erroneous views formed by Dr. Lardner can be explained is, by considering that he dealt with the problem in a general instead of an exact manner. I could not feel any doubt as to the accuracy of my results, but I was not the less pleased to receive a letter from Mr. Freeman, a Fellow of St. John's

But, if we consider the matter rightly, we shall see that this, after all, need not surprise us, since there is already in the enormous distance of Saturn from the sun the amplest reason for believing that he cannot be inhabited by such creatures as exist upon the earth. It is in vain that, by conceiving him to be surrounded by a dense atmosphere, we assign to him a mean climate as warm as that of the earth. The want of direct solar heat still remains, and must be regarded as a fatal objection to the habitability of Saturn by races resembling those with which we are familiar.

In the case of Saturn as in the case of Jupiter, the provision of satellites, and of the rings which form so glorious an object to the astronomer on earth, is altogether inadequate to increase the supply of light received by the Saturnians to any such extent as has been imagined. Those well-meaning persons who insist on their own interpretation of the Almighty's designs, are singularly successful in overlooking very obvious difficulties. If the design of the rings, for instance, really were to compensate the Saturnians for the small amount of light which they receive from the sun, it would surely follow that there was a want of wisdom in the selection of an arrangement by which more light is kept away from Saturn than the rings can possibly reflect to him. And further, during the very season when the extra light derived from the rings is most required by the planet, that is, during the long nights of the Saturnian winter, they exhibit a dark band upon the heavens, concealing whole constellations from the view of the Saturnian people. As far as the satellites are concerned, there is no corresponding difficulty. They undoubtedly reflect the sun's light to Saturn, and, if there really are intelligent beings on the planet, the satellites must undoubtedly present an interesting spectacle, especially when a large number of the moons are nearly full. But a little consideration will show that, even though all the satellites were full at the same time, the quantity of light they could send back to their primary would be wholly inadequate to compensate for the planet's great distance from the sun. According to the best estimates of their magnitude, the eight satellites, taken in their order from the planet, cover spaces

College, Cambridge, stating that he had obtained similar results, and had constructed a table on the plan of Table XI. in my "Saturn" and so closely according with it as not to need separate publication.

on the Saturnian heavens which bear to the space covered by our moon the respective proportion of about 2, 1, 1¼, ¾, ⅝, ⅛, $\frac{1}{100}$, $\frac{1}{40}$. In all, then, they cover an area about six times that of our moon; and as, owing to their great distance from the sun, they are illumined by only $\frac{1}{100}$th of the light which illuminates our moon, they could only send back to the planet, if it were possible for them to be all full together, about $\frac{1}{16}$th part of the light we receive from the full moon. It will be remembered that the light which could be reflected from the Jovial moons, if they could be all full together, bears about the same proportion to our moon's. We seem forced to the conclusion that the satellites were intended to subserve no such design as has been imagined. Here, as in many other cases, the scheme of the Creator is not so obvious to human reasoning as some have complacently supposed.

But we have now to consider peculiarities which suggest that Saturn's globe has not yet reached a condition fitting it to be the abode of living creatures. These peculiarities resemble in great part those which have been already noticed in the case of Jupiter, but a certain most remarkable phenomenon belongs to the ringed planet alone.

The belts of Saturn resemble those of Jupiter in their general shape and also in their color — see the accompanying plate. The dark belts near the equator are of a faint brown or ruddy tinge, those near the pole bluish or greenish gray, while the bright belts are yellowish — the equatorial belt being the brightest of all and almost white. The poles are commonly dusky and even sombre in hue.

The belts change in aspect much as those of Jupiter have been observed to do; and whether we regard the change as due to the bodily transference of the belts of cloud or to the precipitation of their material in the form of rain (while, elsewhere, invisible vapors are condensed into cloud), we are compelled to recognize the action of forces altogether exceeding those which the sun can be supposed to exert upon this distant planet. The light sent to us from Saturn also bears a much greater proportion to the amount of solar light actually received by the planet than is observed in the case of Mars or the moon, and so nearly approaches the proportion noticed in the case of

Jupiter as to lead to the same inference — namely, that a portion of Saturn's light is emitted from the body of the planet.

In these respects, and also in the small density of the planet, we seem to recognize evidence which points to Saturn as probably a heat-sun (if not to any very noteworthy extent a light-sun) to the satellites which circle round him, and not himself the abode of living creatures. Without dwelling further on evidence already fully considered in the case of Jupiter, I turn to one of the most striking facts in the whole range of observational astronomy, as supplying at once new evidence respecting the condition of Saturn and strengthening the evidence adduced respecting Jupiter.

If it can be shown that Saturn's globe is subject to changes of figure perceptible even across the enormous gap which separates him from the earth, it will at once be admitted that he can hardly be regarded as a globe conveniently habitable. Now, I have very little hesitation in saying that evidence of the most conclusive kind exists in favor of this strange mobility of figure. It will presently be seen that it is with the observations of no mere amateur astronomers that I have to deal in endeavoring to establish as a fact that which has commonly been spoken of as an illusion — the assumption by Saturn of his so-called "square-shouldered" figure.

It was in April, 1805, that Sir William Herschel first called attention to this peculiarity. The planet, which had always presented to him an elliptical figure, exhibited a strangely-distorted aspect. A well-marked flattening at the equator, accompanied by an equally well-marked flattening at the poles, gave the planet's globe an oblong figure (with rounded angles), the longest diameters having their extremities in Saturnian latitude 43° 20′— so exactly was the great astronomer able to indicate the nature of the deformity, owing to its well-marked character.

What view shall we form respecting an observation of so remarkable a character? Was the peculiarity due to telescopic distortion? Herschel observed it with several instruments, some seven, some ten, one twenty, and one forty feet in length. Was the phenomenon due to atmospheric disturbances? Such disturbances could not account for a persistent impression, however well they might explain the momentary assumption of the square-shouldered aspect by the ringed planet. Besides, Jupiter presented no such appearance. Was the appearance an optical illusion due to the position of the ring — then slightly open? If so, the planet should always exhibit the square-shouldered aspect when his rings are open to that particular extent; and this is not the case. Besides, we ought to notice a similar illusion, when looking at a picture representing that particular phase of Saturn. Must we, then, accept the astounding conclusion that the giant bulk of Saturn is subject to throes of so tremendous a nature as to upheave whole zones of his surface five or six hundred miles above their ordinary level? Truly the conclusion is one to be avoided, if we can by any possibility find a less startling explanation of the matter.

Yet where are we to look for such an explanation? Was Sir William Herschel simply deceived? I have already considered the general question of illusion, but the reader might entertain the explanation as conceivable that Herschel might for a while have lost the acumen which distinguished him — that illness, for example, might have rendered his observations inexact. But we have abundant evidence that the great astronomer was in the full possession of all his wonderful powers as an observer during the month of April, 1805; we know further that by careful measurements he rigidly excluded all possibility of illusion affecting his judgment.

It would be more satisfactory, doubtless, to the reader, however, to learn that other observers had noticed similar peculiarities, or peculiarities which, if not similar, were at least such as to prepare us to regard the globe of Saturn as liable to remarkable changes of figure. Fortunately, many such observations have been recorded. I take the following from one of an admirable series of papers on Saturn by Mr. Webb, in the *Intellectual Observer* for 1866.

On August 5, 1803, Schröter found Saturn not perfectly spheroidal in figure. Kitchener says that for a few months in the autumn of 1818 he saw Saturn of the figure described by Sir William Herschel, and that with two different achromatics. At this time the ring must have appeared too narrow to account for the appearance as due to illusion. On one occasion the Astronomer Royal had a similar view of Saturn. He remarks, also, that a person unacquainted with Herschel's observation remarked spontaneously on the flattened equator of the planet. On another occasion, Mr. Airy noticed the exact reverse, the planet seeming flattened instead of upheaved, in latitude 45°. In January, 1855, Coolidge, using the splendid refractor of the Cambridge, U. S., Observatory, noticed that the greatest diameter of the globe seemed inclined about 20° to the equatorial diameter; but on the 9th the equatorial diameter seemed the greatest; while on December 6th he says, "I cannot persuade myself that it is an optical illusion which makes the maximum diameter of the ball intersect the limb halfway between the northern edge of the equatorial belt and the inner ellipse of the inner bright ring." All this time the rings were nearly at their greatest opening, so that any illusion should have been of an opposite character to that observed when the rings were nearly closed. In the report of the Greenwich Observatory for 1860-'61, it is stated that "Saturn has *sometimes* appeared to exhibit the square-shouldered aspect." The eminent observers Bond, father and son, have noticed similar peculiarities, using the great Merz refractor already referred to. Each of them noticed a flattening of the north-polar regions of the planet in the summer of 1848, when the ring was turned edgewise toward us. On the other hand, the same observers noticed that in 1855-'57, when the ring was most widely opened, the polar regions did not always seem projected farthest on the outer ring in a symmetrical manner, but four times on the left of the pole, once on the right, and once only, exactly opposite the pole. "The outline of this region also occasionally appeared irregularly flattened and distorted," an appearance not satisfactorily explained by the juxtaposition of the dark shadow of the planet on the ring.

Now, there can be no doubt whatever that the planet Saturn is not ordinarily distorted. In 1832, during the disappearance of the ring, Bessel carefully determined the figure of the planet's disk, and Main in 1848 (when the ring was again turned edgewise toward us) made similar measurements. Each of these trustworthy authorities came to the conclusion that the disk of Saturn did not, at the seasons when they respectively measured it, exhibit any distortion of figure such as Herschel had described.

We seem almost compelled, therefore, to accept the conclusion that the planet Saturn is subject to the influence of forces which either upheave portions of its surface from time to time, or cause vast masses of cloud to rise to an enormous height above the mean layer of Saturn's cloud-envelope. Whichever view we adopt, we cannot fail to recognize the fact that an intense heat must in all probability prevail in the great globe of Saturn; and doubtless the real mass of the planet must emit a brilliant light, though the cloud-strata surrounding him may prevent us from recognizing more than a minute proportion of his luminosity. In fact, according to this view, Saturn and Jupiter, unlike the sun, whose real substance emits a less intense light than the cloud-photosphere surrounding him, must have nuclei — solid or liquid — shining with an altogether more brilliant light than the cloud-envelopes of these planets seem actually to emit.

Why Saturn, rather than Jupiter, should exhibit these mysterious changes of figure, is readily explicable when we remember the near coincidence of the planes in which the Jovial satellites move with the orbital plane of their primary. There thus always results a close agreement between the zone on which the satellites exert their greatest disturbing influences, and that most influenced by the solar action. No such coincidence exists in the case of Saturn, whose satellites travel in a plane inclined nearly thirty degrees to that in which their primary travels. It is worthy of mention, however, that Schröter, an accurate and practised observer, records that on certain occasions he thought he could detect partial flattenings of the disk of Jupiter (see also Preface).

I think the evidence in the case of Saturn favors, at least as strongly as that which has been adduced in the case of Jupiter, the belief that the giant planets outside the zone of asteroids are not themselves suitable abodes for living creatures, but are suns, supplementing the small amount of light, and yet more fully supplementing the small amount of heat which the sun supplies to the satellites which circle around these orbs. Undoubtedly, if we are to judge according to the method which has been so often applied to such questions, if we are to ask ourselves according to what arrangement the central planets and the schemes circling round them seem most reasonably interpreted, we should at once adopt some such conclusion. For, by taking Jupiter and Saturn to be strictly analogous to our own earth, and their satellites to be subsidiary bodies, resembling our moon in this, that they subserve at present no other purpose but to illuminate the nocturnal skies and to sway the oceans of their primaries, we find ourselves perplexed by the consideration that a much simpler arrangement would have subserved these purposes much more completely. In the case of Saturn's satellites, indeed, it seems difficult to conceive that these bodies could have been intended to fulfil any such purposes, since the two outer ones could neither give any useful light to their primary, nor sway appreciably any oceans which may exist upon the planet.

On the other hand, if Saturn and Jupiter are suns to their satellites, we see in the Saturnian and Jovial systems real miniatures of the solar system. We no longer require that the planets themselves should be habitable, any more than we require that our sun should be so. In fine, we do not find in any portion of either system that waste of material which perplexes us under the former arrangement.

I do not say that this mode of reasoning has any great force. On the contrary, I am disposed to demur to the opinion that it is given to man to assign a reason for all things which science may reveal to him. For reasons which seem to me far more convincing, I am led, however, to believe that the two most important members of the planetary scheme must be left without inhabitants for the present, while in exchange I submit to the contemplation of the curious twelve small orbs, constituting two miniature world-systems. The condition of these worlds will be touched on briefly in a separate chapter.

CHAPTER VII.

URANUS AND NEPTUNE, THE ARCTIC PLANETS.

A CIRCUMSTANCE which is of great importance in considering the relations of the outer planets is apt to be lost sight of, owing to the unsatisfactory manner in which, in nearly all books on astronomy, the planetary orbits are represented. To look at the series of equi-distant and concentric circles representing the orbits of the planets, who would suppose that, in passing from the orbit of Jupiter to that of Saturn, a distance five times as great as that which separates our earth from the sun has to be traversed? But the distance separating Uranus from Saturn is twice as great even as this tremendous gap, while Neptune travels as far beyond Uranus as Uranus beyond Saturn. Nine hundred millions of miles in width is the enormous gap by which the path of Uranus is separated from that of the ringed planet on the inner side, and from that of distant Neptune on the outer, so that a line equal to the diameter of Jupiter's orbit would barely suffice to reach from Saturn to Uranus, or from Uranus to Neptune, even when either pair of planets are in conjunction.

We know so little of the physical aspect of Uranus and Neptune that it is extremely difficult to form any opinion as to their condition. The two planets resemble each other in size, each being far smaller than either of the giant orbs we have lately been considering. Uranus has a diameter of about 33,250 miles; Neptune is somewhat larger, his diameter having been estimated at 37,250 miles. The volume of Uranus is 74, the volume of Neptune 105 times that of the earth. Both planets exceed Saturn in density; for, whereas Saturn's mean specific gravity is but $\frac{13}{100}$ths, that of Uranus is $\frac{17}{100}$ths, and that of Neptune $\frac{18}{100}$ths, of the mean specific gravity of our globe. Thus each planet has a density nearly equal to that of water. The mass of Uranus exceeds the earth's about 12½ times, while that of Neptune is some 16¾ times as great as the earth's. It will be seen, therefore, that though these two far-distant worlds are much less massive than Jupiter or Saturn, each of them outweighs many times the combined mass of the four planets which travel within the zone of asteroids. Yet gravity on the surface of these two orbs is but about three-fourths of terrestrial gravity.

The disk of the sun as seen from Uranus is less than that which we see in the proportion of nearly 390 to 1, while the Neptunians have a sun only about $\frac{1}{900}$th of ours, in apparent size; and in these proportions the solar light and heat received by these planets are respectively diminished. So small does the sun appear, in fact, that to eyes such as ours his orb could not present a disk-like figure, but would appear like an exceedingly brilliant day-star.

So far we have found the circumstances of the two planets somewhat similar. But we have now to consider a relation presented by Uranus, which is not shared in by Neptune. It may be remarked that we know so little about either planet that any very careful consideration of their habitability would be simply a waste of labor. The evidence I am about to adduce, however, in the case of Uranus, seems thoroughly to dispose of the claim of this planet to be regarded as a world inhabited by creatures resembling those we are acquainted with on earth; and, as we cannot reasonably suppose Neptune to be inhabited by such creatures while Uranus is not, we may very fairly regard the question as disposed of for both planets, even though the relation dealt with is peculiar to Uranus.

We know that in the case of Jupiter, as in that of Saturn, the position of the plane near which the satellites travel is nearly coincident with the plane of the primary's equator. Therefore, though no telescope has yet exhibited any features of the disk of Uranus which can enable us to determine the position of its equator, we can reasonably infer from the motion of the satellites how the equator of the planet is situated.

Now, the satellites of Uranus travel in a plane very nearly at right angles to the plane in which the planet travels. It may be mentioned also, though not important for my present purpose, that they travel in a retrograde direction. We conclude, then, that the axis of Uranus lies very nearly in the plane wherein the planet moves around the sun, and that the planet rotates in such a way around this axis that the sun moves across the Uranian skies from west to east, instead of from east to west. The latter relation is of no great importance; the former, however, involves results which dispose at once, and thoroughly, of any hopes we might entertain of discovering creatures in Uranus resembling those which inhabit the earth.

The inclination of the plane of Uranus's equator to the path in which he travels being about 76 degrees, it follows that the Uranian sun has a range of about 76 degrees on either side of the celestial equator, during the long Uranian year. Already, in considering the seasons of Venus, I have dealt with a peculiarity of this sort; but in the case of Uranus the effects are more serious. We have only to consider what would be the result of so wide a range of solar excursion north and south of the celestial equator in a latitude corresponding to that of London, to see how importantly the climatic relations of a planet like Uranus, occupying eighty-four years in circling once round the sun, must be affected by such a peculiarity. We know that in the latitude of London the sun reaches at noon, in spring or autumn, an elevation of about 38½ degrees above the southern horizon, that in summer he passes the meridian 23½ degrees higher, while in winter he passes the meridian 23½ degrees lower, or only fifteen degrees above the horizon. But in a similar Uranian latitude, while the sun would reach the same meridian elevation in spring or autumn, he would in summer travel throughout the day in a small circle, fourteen degrees only from the pole, raised of course 51½ degrees above the northern horizon. And obviously, since the year of the Uranians lasts eighty-four of our years, the continuance of the sun above the horizon would last for many years.* So far there is nothing to render life in Uranus unpleasant, always supposing the small amount of light and heat supplied by the sun to be compensated by some such atmospheric arrangements as physicists have thought necessary for the convenience of the more distant planets. But, when we consider the nature of the Uranian winter, we find the circumstances such as no such arrangements can be conceived to alleviate. The winter path of the Uranian sun, in a latitude corresponding to that of London, is just as fully pressed below the horizon as the summer path is raised above it. At midnight the sun is 65½ degrees, at nominal noon he is 37½ degrees below the southern horizon. And as with the summer day, so with the winter night, years elapse before either comes to an end. For upward of twenty years, in a latitude corresponding to that of London, the Uranians — if there are any — never see the small Uranian sun. During all this long time, too, a sight even is denied them of all parts of the solar system, interior to the orbit of Uranus; though this deprivation cannot be regarded as very serious when it is remembered that to such eyesight as ours Saturn could barely be visible from Uranus, even when most favorably situated,* while Jupiter, always near the sun, could only be occasionally seen, shining with a light somewhat less than a fiftieth of that which he reflects to us when in opposition.

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* Exact calculation applied to relations so uncertain as those here in question would be out of place. From a careful construction, however, with 76° as the assumed value of the inclination of the equator of Uranus to the plane of his orbit, I find that the sun would continue above his horizon in summer for about 23¼ years. Of course, it follows that the sun would continue below the horizon for an equally long period in winter!

When we consider other latitudes, we still find Uranus ill provided for as respects his winter season. In all latitudes nearer the pole than the latitude just considered, the Uranians have winters lasting from twenty years to upward of forty. In latitudes nearer the equator the winter night is shorter, but we must approach quite close to the equator before we reach a latitude where the winter night lasts less than a year or so. Over a belt extending about fourteen degrees on each side of the equator there is a perennial succession of days and nights never exceeding the full duration of the Uranian diurnal rotation. But we must not suppose that we have thus found an Elysian zone in Uranus. The immense range of the sun's excursions produces here also a variety of seasonal changes which we should find altogether unendurable. From a sun barely rising above the horizon in winter, to a sun which rises vertically overhead twice in the course of the Uranian summer, is a change which hardly accords with our views of what is desirable in the progress of the seasons. At the equator itself there are in reality two summers, occurring at the period of the sun's passing the celestial equator. Here for many years together the sun passes day after day to a point nearly overhead. But then comes the long winter, in the heart of which the sun rises barely fourteen degrees above the northern or southern horizon. By whatever arrangement we render the long Uranian winters in this part of the planet endurable, we render the heat of his long summers unendurable; and *vice versa,* if we conceive of atmospheric relations which would render his summers pleasing, we have caused his winters to be so intensely cold that no creatures we are familiar with could endure the prolonged and bitter frosts, contrasting so distressingly with the imagined geniality of his summer weather.

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* Admiral Smyth speaks of Saturn as a fine morning and evening star for the Uranians; but, though Saturn may be visible, he can hardly be a fine object. At his elongations he is twice as far from the Uranians as he is from us when in opposition, and further he presents but a half disk. His light must in fact be reduced to less than one-eighth of that which he presents to us when in opposition; and, as, instead of being on a black sky, he must be always seen from Uranus on a twilight sky, he cannot appear a very fine object.

If Uranus be inhabited at all, then, it must be by creatures constituted in a very different manner from any with which we are acquainted. To such creatures, if any among them be gifted with intelligence, the heavens, though not adorned with planets, must yet present an interesting subject of study. The position of the pole, lying close by the zodiac, so that among the zodiacal constellations there must be all the varieties of motion which we recognize in passing from the equatorial to polar constellations, would lead to a certain complexity in celestial charts and globes, which would invite us to the conclusion that the Uranians must be capital mathematicians. Then there are certain astronomical subjects of study to which their mathematical powers may be devoted perhaps more successfully than those of our astronomers. For example, the wide sweep of the planet's orbit would enable the Uranians to recognize a displacement of the stars in the course of the long Uranian year. The star Alpha Centauri, which only exhibits to the terrestrial observer an annual parallax of one second, would exhibit to the observer in Uranus a displacement of about the third part of a minute. Other stars would be affected in like proportion, and perhaps the Uranians may thus be enabled to form some conception of that relation which hitherto has proved too baffling a problem to our astronomers — the actual configuration of the nearer parts of the sidereal system.

The Neptunians would of course be even more favorably circumstanced.

One difficulty presents itself, however, in thus considering the prospects of the Uranian and Neptunian astronomers. The enormous length of the year of each planet requires that either the astronomers in Uranus and Neptune should be very long-lived, or that they should be very enthusiastic in the cause of science, to prosecute singly such observations as Henderson, Olbers, or Peters, have singly prosecuted on our earth. A Uranian who made one set of observations to determine stellar parallax when he was, say, twenty-five years old, would have to wait till he had nearly reached the threescore years and ten (not perhaps allotted as the span of Uranian life) before he could make the corresponding set, by comparing which with the former stellar parallax was to be determined. In Neptune life must be prolonged over the century (unless the study of observational astronomy commence during the babyhood of the Neptunians) in order that a complete set of observations for determining stellar parallax should be carried out. One cannot but conceive that a certain sluggishness must mark the progress of astronomy in these far-off worlds under such circumstances. In fact, the mere consideration that, after a constellation has passed away from the nocturnal skies of Uranus or Neptune, thirty or forty years in one case, and seventy or eighty in the other, must pass before the constellation again becomes favorably visible, suggests characteristics of astronomical observation altogether different from those we are familiar with.

Admiral Smyth suggests that these distant planets must be convenient outposts for watching the approach or recession of comets; but, with all diffidence, I would venture to point out that the inhabitants of the earth are, on the whole, more favorably situated in this respect. Every large comet which approaches tolerably near to the sun during perihelion passage is as likely to be seen as to be missed by the inhabitants of earth; but scarcely one out of a thousand such comets would be seen from Uranus or Neptune, since, to be visible, a comet must approach the sun or recede from him along a course passing tolerably near to the particular position of either planet at the time; and the chances in the case of any individual comet would be enormously against such a contingency.

With eyesight such as ours the Uranians could distinctly see Neptune when in opposition, but the Neptunians would be wholly unable to see Uranus, or indeed any known planet of the solar system.

Perhaps, though we have very little evidence on the point, it will be thought more reasonable to suppose that Uranus and Neptune are suns to their respective systems of satellites, than to imagine that these two drearily-circumstanced planets are themselves inhabited. Their satellites cannot possibly compensate, to any noteworthy extent, for the small amount of solar light or heat which reaches their primaries. On the other hand, it is not difficult to conceive that the planets may afford an important supply of heat (at any rate) to their dependent orbs. Certainly, so far as the evidence we have extends, Uranus and Neptune resemble Saturn and Jupiter too closely not to warrant the application of any arguments deduced from the appearance of the two giant planets to the case of their inferior but still gigantic brethren.

Viewing the matter thus, we seem led to the conclusion that the planets which lie outside the zone of asteroids are distinguished from those within that belt, not merely, as had so long been recognized, in the attributes of size, density, rapidity of rotation, and complexity of the systems circling round them, but in this more important and more interesting circumstance, that they and their dependent orbs are real miniatures of the solar system. Four suns they would seem to be, not indeed suns resplendent like the primary sun round which they travel, but still giving out perhaps no insignificant supply of light; not heated to incandescence as he is, but still supplying an amount of heat proportionately far greater than the quantity of light they give forth: in fine, not, as he is to the inner planets, the sole source whence all supplies of force are derived, but adding their influence to his in a variety of complicated but doubtless well-ordered combinations, in such sort that the small worlds which circle around them are provided with all that is needful to the well-being of their inhabitants.

CHAPTER VIII.

THE MOON AND OTHER SATELLITES.

ALTHOUGH I do not think that the moon can be regarded as probably at present the abode of life, there are many reasons for studying in a work on other worlds the various relations she presents to us. In the first place, she subserves various useful purposes in the economy of our own earth; then there are circumstances in her appearance which suggest that at one time there may have been life upon her surface; and, lastly, she affords us the only information we have concerning the probable relations presented by the noble systems of moons which circle around Jupiter and the other planets outside the orbit of the asteroids.

Now, with regard to the present habitability of the moon, it may be remarked that we are not justified in asserting positively that no life exists upon her surface. Life has been found under conditions so strange — we have been so often mistaken in assuming that *here* certainly, or *there,* no living creatures can possibly exist — that it would be rash indeed to dogmatize respecting the state of the moon in this respect.

Still, in the case of the moon we have relations wholly different in character from those we have hitherto had to consider. We no longer have to deal with a question of the various degrees of heat and cold, of atmospheric rarity or density, and the like, but with relations which do not in the slightest degree resemble those we are familiar with on earth.

In the first place, the moon has no appreciable atmosphere. We have long known this quite certainly, because we see that when stars are occulted by the moon they disappear instantaneously, whereas we know this would not be the case had the moon an atmosphere of appreciable extent. But if any doubt could have remained, the evidence of the spectroscope in Mr. Huggins's hands would have sufficed to remove it. He has never been able to detect a sign of the existence of any lunar atmosphere, though Mars and Jupiter, so much farther from us, have afforded distinct evidence respecting the atmospheres which surround their surface.

Then, secondly, there are no seas or oceans on the moon. Were there any large tracts of water, the tremendous heat to which the moon is subjected during the course of the long lunar day (lasting a fortnight of our time) would certainly cause enormous quantities of water to evaporate; and not only would the effects of this process be distinctly recognizable by our telescopists, but the spectroscope would exhibit in an unmistakable manner the presence of the aqueous vapor thus formed.

Thirdly, there are no lunar seasons. The inclination of the moon's axis to the orbit in which she travels round the sun is nearly 89°, and with this inclination there can be no appreciable seasonal changes.

Fourthly, the enormous length of the lunar day is altogether opposed to our conceptions of what is suitable for animal or vegetable life. The lunar day lasts about a fortnight, and the lunar night is, of course, equally long. Were this all, the inconvenience of the arrangement would be unbearable by beings like ourselves. But far more serious consequences must result from the combination of the arrangement with the want of an atmosphere; for whereas during the lunar day the surface of the moon is exposed to an inconceivably intense direct heat, undoubtedly sufficient to heat that surface far above the boiling point, during the lunar night the heat is radiated rapidly away into space (no atmosphere checking the process), and an intensity of cold must prevail of which we can form but imperfect conceptions.*

The mere fact that our earth is always invisible to threesevenths of the moon's surface is one which points very strongly to the conclusion that the present condition of the moon is not the one best calculated to meet the wants of living creatures on her surface. In long-past ages, when her rotation had not yet been forced into accordance with her revolution* (as at present), the earth must have subserved a variety of most important purposes. If water then existed on the surface of the moon, the earth must have raised tidal waves in her oceans. She must further have reflected enormous supplies of light and heat toward her dependent orb, even if at that time she were not a secondary sun for the lunarians. She must have travelled across the lunar skies as the moon travels over ours, presenting a variety of interesting and beautiful phases affording useful time-measures, and so enabling the travellers on the moon in those long-past ages to guide their course in safety over her oceans or her deserts. But now she is invisible from a large portion of the moon's surface, and almost a fixture in the skies of those parts, even, of the moon whence she can be seen. Were there lunar oceans, she could raise no tides in them. Were there a lunar atmosphere, she could shed no heat, to be garnered up, so to speak, by that atmosphere, and to compensate, in some sort, for the long absence of the sun.

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* The moon's physical habitudes are in fact so very different from those of the earth that one cannot read without astonishment the well-known passage in which Sir W. Herschel pleads for the moon's habitability. "Its situation, with respect to the sun," he says, "is much like that of the earth, and by a rotation on its axis it enjoys an agreeable variety of seasons (!) and of day and night. To the moon, our globe will appear to be a very capital satellite, undergoing the same regular changes of illumination as the moon does to the earth. The sun, the planets, and the starry constellations of the heavens, will rise and set there as they do here, and heavy bodies will fall on the moon as they do on the earth. *There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.*" The evidence is, however, all the other way.

But have we evidence that at some far-distant epoch the moon was inhabited? Taking for our guidance the analogies which are available to us, can we really conclude that once, in all probability, those barren wastes were clothed with vegetation, those dreary solitudes the abode of life?

When we contemplate with attention the lunar surface, considering the indications it presents of past activities, we are led to inquire how the forces which have been so busily at work were expended. If Nature, studied thoughtfully, teaches us the lesson that there is no form of force which is not the representative of some other preacting form of force, she also teaches us that no form of force ever works without generating other forces as its own energies are expended. The meteor which sweeps with planetary velocity through space may be brought to rest upon the sun, but the energy stored up in its motions is not wasted; the sun may expend the stores of force he derives from meteoric impact, but not idly;* all round us we see the fruits of solar energies, we feel them within ourselves, we exert them upon others. And, therefore, when we see on the moon signs that her surface was at one time upheaved by tremendous volcanic forces, we are led to the conclusion that between the era when she was thus disturbed, and the present time, when she seems absolutely quiescent, there must have been a period when her energies were employed in sustaining various forms of life. There *has,* in this instance, been a process resembling exhaustion, though we know the forms of force which have passed away from the moon have not really ceased to exist; but before the lunar forces were dissipated into space, so to speak, they must have subserved that great purpose which seems the end of all Nature's workings — the support of life.

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* The researches of Adams into the peculiarity of the moon's motion, called her acceleration, suffice to show that, under the influence of the moon's attraction on our oceans, the earth's rotation is gradually diminishing; so that, though many millions of ages must elapse first, she will one day so rotate as to keep always the same face turned toward her satellite. We cannot doubt that it has been by a process of this sort that the moon's rotation has been brought to its present rate. In fact, independently of the evidence afforded by the earth's gradual loss of rotation, we cannot account for the moon's peculiarity of rotation without regarding it as due to the earth's controlling influence. A perfectly homogeneous sphere, started on a direct line at the moon's distance, and with the same velocity, would travel without rotation on an orbit like the moon's, and would thus in completing a revolution exhibit every part of its surface to us.

Associated, however, with this subject, there are questions of a perplexing character, which invite our careful consideration. If life ever existed on the moon, that orb must have possessed an atmosphere and seas. Independently, also, of our views on the subject of life upon the moon, we are led, by the revelations of the spectroscope respecting the solar system, to believe that all the bodies within that system are in a general sense similarly constituted; and, if this be so, there must once have been oceans and air upon the moon. What has become of the moon's atmospheric envelope, and of the lunar oceans?

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* The question may be asked, What becomes of the immense supplies of light and heat continually poured by the sun and other stars into space? We cannot tell; yet we know certainly that they cannot be wasted. The heat of Arcturus, measured by Mr. Stone, gives an account of one large portion of the stellar heat-supplies, because we know that, small as the amount we receive may be, we must multiply that amount millions on millions of times to get the total received by all the orbs in space from this particular sun. But we know that a large portion of our sun's light and heat must either fail to fall on any other orbs, or must be gradually exhausted in its progress through space (for, if lines from the sun in every direction encountered orbs, the sky ought to be lighted up at all times with star-splendor — which is no other than sun-splendor). In either case we cannot tell what becomes of the portion seemingly wasted, though in the latter case we may affirm confidently that there is simply a change in the nature of the force. In both cases we know that the total of force in the universe remains undiminished. There is, indeed, a seeming contradiction here; but it is not different in character from the seeming contradictions suggested by the consideration of infinite space and infinite time, which yet we are compelled to recognize as absolutely as finite space or finite time.

In four several ways this question has been answered. Some have thought that the oceans and air have been withdrawn into cavities within the moon's substance. Others have imagined that the air and oceans may have passed away to the farther hemisphere of the moon. According to a third theory, a comet has carried off the lunar oceans and atmosphere. And, lastly, a fourth theory has been maintained, according to which the lunar air, and *a fortiori* the lunar seas, have been changed by intensity of cold into the solid form.

Of these theories, the first and last only seem worthy of consideration. We see so much of the moon's farther hemisphere during her librations that we must perforce reject the second, even if we had any trustworthy analogy for believing so strange an arrangement to be possible.* The third theory is opposed by all that modern astronomy teaches respecting the constitution of comets.

The theory that an atmosphere formerly surrounding the moon has passed with the lunar oceans into the interior of our satellite has been supported by physicists of considerable eminence. The relatively low specific gravity of the moon (little more than half the earth's) suggests the possibility that cavities large enough to contain even all the waters of our own oceans may exist within the moon. Nor does the fact that we can see no unmistakable signs of chasms extending deep into the moon's substance suffice to render the theory untenable, or even improbable. It is difficult to understand how the inrush of the waters took place. Certainly it cannot have happened while the moon's volcanic forces were in vigorous action; yet a period must undoubtedly have arrived when by little and little the waters could retire within the moon's substance without being vaporized. From what we know of volcanic action on the earth, the lunar volcanoes must have drawn fresh supplies of energy from the gradual influx of water; and one can thus understand why the aspect of the moon indicates that, up to the last moment, so to speak, of her existence as a world, the forces upheaving her crust were busily at work. We can thus see how it has come to pass that the moon's surface shows so few signs of the action of rain or running water.

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* Prof. Newcombe, of America, has shown excellent reasons for doubting whether even that displacement of the moon's gravity, on which the theory has been based, can be admitted as an established fact. Independently of this, however, the theory will not bear examination. Any one who will draw a cross-section of the moon (in a plane passing through the earth), and endeavor to assign such a position to an atmosphere of moderate extent that, even during the moon's extreme librations, no signs of the atmosphere could be perceptible from the earth, will at once see that the theory is untenable.

The theory that the lunar oceans have become frozen, and that afterward even the gases forming the lunar atmosphere have become solidified, was maintained by Buffon and Bailly in the last century, and has been supported by several astronomers in our own day. In some respects, the aspect of the moon (especially the absence of well-marked colors from her surface) seems to favor the theory. Nor need the excessive heat to which the moon's surface is exposed for weeks at a time be considered a sufficient reason for rejecting it, because we have no means of judging how that heat would act where there is no atmosphere to prevent its immediate and entire reflection into space. We know that, despite the intense heat which is poured upon the summits of the Himalayas, the snow there — though a portion may melt during the day — remains year after year and age after age undiminished; and on the summit of the Himalayas the atmosphere is dense and heavy compared with that which exists even in the lowest abysms of the lunar ravines. If absolute reliance be placed on the results which have been deduced from the application of the great Parsonstown mirror to the measurement of the lunar heat, it would seem as though we must abandon the belief in the existence of frozen oxygen or nitrogen on the moon's surface, since, according to those results, a large proportion of the moon's heat is radiant — in other words, the moon's surface has been actually raised to a high degree of heat by the solar rays. At present, however, physicists are not prepared to look with perfect confidence on the method by which, in the researches made at Parsonstown, an attempt has been made to distinguish between the heat which the moon reflects and that which she radiates into space.

On the whole, however, the former theory seems to have the strongest evidence in its favor, or rather the least decisive evidence against it.

In considering the systems of bodies which circle around the outer planets, we are struck at once by several marked circumstances of contrast between their condition and that of our own moon.

In the first place, we have no satisfactory evidence that the satellites of Jupiter and Saturn turn always the same face toward their primary. It is true that Sir William Herschel was led by certain observations of the satellites of Jupiter to conclude that this relation holds in their case. But we have far stronger evidence against such a view, in the fact that modern observers, armed with telescopes of the most exquisite defining powers, have not only been unable to confirm the relatively rough observations made by Herschel, but have noticed peculiarities of appearance only explicable by the theory that the rotation of the satellites is quite independent of their motion of revolution around Jupiter. Dawes, for instance, has observed that the markings seen on the third satellite, when transiting Jupiter's disk, are variable. Bond has seen this satellite as a well-defined black spot on certain occasions, while on others it has appeared quite bright on the disk of the planet. He once saw this satellite bright as it entered on the disk of Jupiter, and about half an hour later as a dark spot; while Mr. Prince, with a powerful reflector, has seen the satellite dark first and afterward bright. It need hardly be said that, if the satellite turned always the same face toward its primary, no such varieties of appearance would be presented during transit. The following passage from Webb's "Celestial Objects" points strongly also to the conclusion that the rotation of the Jovial satellites must be independent of their revolution. After mentioning that the variable light of the satellites may be caused by the existence of spots upon their surface, he proceeds: "A stranger source of anomaly has been perceived — the disks themselves do not always appear of the same size or form. Maraldi noticed the former fact in 1707, Herschel ninety years afterward inferring also the latter, and both have since been confirmed. Beer and Mädler, Lassell and Secchi, have sometimes seen the disk of the second satellite larger than that of the first, and Lassell, and Secchi and his assistant, have distinctly seen that of the third satellite irregular and elliptical; while, according to the Roman observers, the ellipse does not always lie the same way."

It will easily be seen that these peculiarities indicate the existence of dark markings on these bodies, and that, as the satellites rotate, the varying position of these markings causes the satellites seemingly to change in figure, since the brighter part of the satellite would be that which would determine its apparent figure. And further, since the change of figure shows no correspondence with the position of the satellites in their revolution, we infer that their revolution is independent of their rotation.

It is worthy of notice, however, that even if the inner satellites turned always the same face toward their primary, the peculiarity would not (as in the case of our moon) result in an inordinate lengthening of their diurnal period, since Jupiter's two inner satellites complete a revolution in one day eighteen and a half hours, and three days thirteen hours respectively; while the revolutions of Saturn's five inner satellites are severally accomplished in twenty-two and a half hours, one day nine hours, one day twenty-one hours, two days eighteen hours, and four days twelve and a half hours.

So far as we can judge from Laplace's estimates, the specific gravity of Jupiter's moons must be very small indeed, ranging from one-ninth to four-fifths of the specific gravity of water. But very little reliance can be placed on these results, because the only evidence we have respecting the mass of the satellites is that founded on the perturbations to which their motions are subjected, and it is very difficult indeed to estimate these perturbations. When to this we add the circumstance that little reliance can be placed on measurements of the minute disks presented by the satellites, it will be seen that our estimate of the specific gravities of these bodies cannot by any means be regarded as trustworthy.

As seen from his satellites, Jupiter must present a magnificent scene. To the inhabitants, if such there be, of the innermost satellite, he exhibits a disk nearly twenty degrees in diameter. Thus, whereas there might be about seven hundred moons such as ours placed all round our horizon, the disk of Jupiter, as seen from the inner satellite, could occupy a full eighteenth part of the horizon's circumference. The disk of

Jupiter, as so seen, would cover a space on the heavens exceeding more than fourteen hundred times that which our moon covers. To the second satellite, Jupiter presents a disk about 12½ degrees in diameter, or about six hundred times as large as our moon's. To the third satellite he shows a disk about 7¾ degrees in diameter, or more than two hundred times the size of the moon's. And, lastly, the inhabitants even of the farthermost satellite see him with a diameter of about 4½ degrees—that is, with a disk more than sixty-five times as large as that of our moon. So that, if the views I have put forward respecting Jupiter be correct, the enormous space he covers on the skies of his respective satellites must suffice to compensate in part for the relatively small amount of heat which he can be supposed capable of emitting.

If the satellites rotate with a motion independent of their revolution, Jupiter passes across their skies like a vast moon, exhibiting phases such as those presented by ours, but on a far vaster scale. But, besides his phases, he must exhibit to the inhabitants of his satellites the most marvellous picture that can be conceived. His belts' changes of figure and color, only rendered visible to our astronomers by powerful telescopic aid, must be distinctly visible to creatures on his satellites, and cannot but afford reasoning beings on those orbs a most astounding theme for study and admiration.

To the inhabitants of the satellites which circle round Saturn, the ringed planet must present an even more interesting spectacle. His disk, as seen from the nearest of his satellites, has a diameter of 17 degrees, and an apparent surface exceeding more than nine hundred times that of the moon. From the farthest satellite his disk is less than a degree in diameter, and therefore not quite four times as large as our moon's. Between these limits the apparent size of Saturn varies as we pass from satellite to satellite; but from the sixth satellite his apparent surface is twenty-five times, while from the seventh it is sixteen times as large as the moon's; so that the outer satellite is quite exceptionally circumstanced in this respect.

It is not so much from the apparent size of his disk, however (though in the case of all the inner satellites that must be a most remarkable relation), as from the peculiar character of his ring-system, that Saturn must derive his chief interest. It is true that the inner satellites travel nearly in the plane of the rings, so as always to see them nearly edgewise. But, even so viewed, the rings must present a most striking appearance. From the inner satellite, indeed, the extreme span of the ring-system must be more than 90 degrees;* so that when one extremity is seen on the horizon the system would appear as an arch thickest in the middle, extending over an arc of about 93 degrees, and having the disk of Saturn at its centre. When the whole of this arch is illuminated, Saturn is "full"; at other times he presents all the phases shown by our moon, and the arch of light is correspondingly shortened. Saturn "full" and in the zenith, with the ring-system dependent on either side of his disk, must be a glorious spectacle as seen from certain regions of his innermost satellite. The display would diminish in grandeur, though not perhaps in interest, as seen from satellites farther and farther away. But the inhabitants of the outermost satellite of all have the privilege of seeing the Saturnian ring-system opened out much more fully than as seen from the other satellites, since the path of this moon is inclined some 15 degrees to the plane of the ring.

Of the satellites of Uranus and Neptune little can be said, because so little is known either respecting these orbs themselves or their primaries. I may remark that, despite the evidence brought forward to the contrary, I have very little doubt that Uranus has at least eight satellites. Four of those discovered by Sir W. Herschel have not indeed been yet identified; but one cannot read the account of his method of procedure without feeling that no amount of mere negative evidence can be opposed effectively to the positive information he has left respecting these four orbs. Indeed, when we remember that Uranus is twice as far from us as Saturn, while it has only been in recent times that the eighth Saturnian satellite (the seventh in position) has been discovered, we cannot but consider that in all probability many more Uranian satellites will one day be discovered. Neptune also, no doubt, has a large family of satellites circling around him.

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* About 93° according to the best estimates of the dimensions of the rings and the distance of the satellite.

CHAPTER IX.

METEORS AND COMETS: THEIR OFFICE IN THE SOLAR SYSTEM.

THERE are few more interesting chapters in the history of astronomy than that which deals with the gradual introduction of meteors into an important position in the economy of the solar system. Regarded for a long time as simply atmospheric phenomena (though many ancient philosophers held another opinion), it has only been after a long and persistent series of researches that they have come at length to be regarded in their true light. But, though the history of those researches is not only full of interest, but highly instructive and encouraging, this is not the place for entering at length into its details. I must present facts and conclusions, rather than the narrative of observations or calculations by which those facts and conclusions have been established. Nay, it would seem at first sight as though even the nature of meteors could have very little to do with the subject of this treatise, since we cannot suppose these small bodies to be inhabited worlds. It will be found, however, that, though this is certainly true, there are reasons for believing that meteors are associated in a very intimate manner with the general relations of the scheme of worlds forming the solar system.

Under the head "Meteors" I include all those objects which reach the earth's atmosphere from without, whether they actually make their way to her surface unbroken, like the aërolites; or explode into small fragments, as bolides and fire-balls have been observed to do; or are apparently consumed in traversing the upper regions of the air, as happens with shooting or falling stars. All these objects, we now know, represent in reality bodies of greater or less size, which, before their encounter with the earth, were travelling around the sun in orbits of greater or less eccentricity. The larger masses, though they must be very numerous (or our earth would not once in many ages encounter any of them), are yet relatively few in number as compared with fire-balls, and still more so in comparison with shooting-stars. It has been calculated, indeed, that these last are so numerous that the earth, in passing through a region of space equal to her own dimensions, must encounter no less than thirteen thousand of them; while of yet smaller bodies, whose passage through our air would only be recognizable by telescopic aid, she is supposed to encounter as many as forty thousand within a similar space. Without laying great stress on these calculations, we may yet feel quite sure that the earth must encounter enormous numbers of these bodies, from the mere fact that, though at any fixed station but a minute slice (so to speak) of the earth's atmosphere is within view, and even but a portion only of that slice visible to a single observer, six or seven falling stars on the average may be seen during each hour of the night.

It will be seen, then, that a problem of the utmost importance was involved in the question whether these bodies came from the interplanetary spaces, or from the region of space over which the earth's own attractive energies prevail. Now that we know the former view to be the true one, we recognize the fact that, though each meteor may be individually insignificant, the meteors of the solar system, looked on as a single family, form a highly-interesting and important portion of the solar system.

But now a yet more significant relation has to be considered. Regarding meteors as planetary bodies, they might yet be relatively unimportant, if we had any reason to believe that they form a sort of zone or belt near the earth's orbit, resembling in a sense the asteroidal zone, only composed of far smaller constituent bodies. We could not *then* argue, from the number of meteors encountered in a given time by the earth, the largeness of the total number of these bodies; for it might well be that this zone had no counterpart, either in the outer part of the planetary system or within the orbit of the earth. What has actually been discovered, however, respecting the paths along which the meteoric bodies have reached the earth, immensely enhances the importance of these objects.

It has been proved, on evidence perfectly incontestable, that two well-marked meteoric systems travel in orbits of enormous eccentricity. The August meteors travel on a path so eccentric that in the neighborhood of the earth's orbit it may be regarded as almost parabolic in figure. That it is not absolutely parabolic is shown, of course, by the fact that a period has been assigned to the revolution of the members of the zone. No observations have been indeed made by which astronomers could determine the orbit of these meteors, since for this purpose an exact determination of the velocity with which they enter the earth's atmosphere would be requisite, while the observations actually made to determine their velocity are confessedly inexact. But an association, altogether too close to be regarded as accidental, has been discovered between their orbit and that of a bright comet which appeared in 1862, and this, combined with what has since been established respecting the relations between comets and meteors, enables astronomers to adopt quite confidently the orbit of the comet as that of the meteoric system. Now, a period of one hundred and forty-five years implies, according to Kepler's law, an orbit having a mean distance nearly equal to that of Neptune. And since the orbit is so eccentric as to bring these bodies close by the earth when they are near perihelion, it follows that their aphelion distance must exceed their mean distance in the same degree. Hence the aphelion point of the August meteors must lie nearly twice as far away from us as the orbit of Neptune.

The November meteors have been shown in like manner to travel in a period of thirty-three and a quarter years around the sun, the aphelion of their orbit lying far beyond the path of Uranus.

So far, then, as we can judge from the only two meteoric systems whose orbits can be said to have been satisfactorily determined (though there are many other systems which have been associated with known comets), we are led to the conclusion that the meteoric orbits are for the most part eccentric. We know, further, that they are inclined in all directions to the plane in which the earth travels, because we see that their constituent bodies fall upon the earth in directions which show no tendency to near coincidence with the ecliptic.

Now, these two circumstances are full of meaning. If the meteors travelled in nearly circular orbits, at a mean distance nearly equal to the earth's mean distance from the sun, then the earth would be certain to encounter meteors in the course of her orbital motion round the sun. Again, if the meteors travelled in eccentric orbits, whose perihelia lay within the earth's orbit, and if these orbits all lay in or near the plane of the earth's path, the earth could not fail to encounter meteors as she travelled round the sun. But under the actual circumstances — the mean distances of the meteoric orbits being in no way associated with the earth's mean distance, and the inclination of these orbits to the ecliptic not being in any way limited — the two questions are at once suggested: 1. What is the *a priori* chance that the earth would encounter the members of any meteoric system taken at random? and, 2. If this chance be small, what is the conclusion to be drawn from the fact that the earth encounters meteors belonging to many systems?— the number already recognized being nearly sixty. Now, assigning elements at random to a meteor-system, we see that, unless the resulting orbit actually coincides with the plane of the ecliptic (a relation which would not happen in a million trials), the orbit will intersect that plane in two points, lying on a straight line through the sun. And, for the earth to encounter members of the meteoric system, it is requisite that one or other of these two points shall lie close to the earth's orbit. But these points may have any position whatever in the plane of the ecliptic, and the chance that one of them has the requisite position may be regarded as indefinitely small. It follows, then, that the *a priori* chance of the earth's encountering the members of a meteoric system is indefinitely small; and hence we conclude that the number of meteoric systems she passes wholly clear of is indefinitely great, in comparison with the number whose members she encounters. But she actually encounters meteors belonging to no less than fifty-six systems: hence the total number of meteoric systems belonging to the planetary scheme must be an indefinitely large multiple of the number fifty-six, or, in other words, it must be enormously beyond our powers of conception.

But this being so, it behooves us to inquire, first of all, what extent we must assign to individual meteoric systems, and how densely we may suppose meteoric masses to be strewn along each system; and, secondly, what may be the nature, quality, and substance of these meteoric masses. For we clearly begin to see that we are in the presence of relations which may — or, I should rather say, which must — affect most importantly the economy of the solar system.

Now, we have seen something already of the longitudinal extent of meteoric systems, since that extent corresponds to the circumference of meteoric orbits, and we have seen that these orbits have enormous dimensions. We may, indeed, suppose that in some cases the whole extent of an orbit is not occupied by meteoric masses at any one instant; but even when, as in the case of the November meteors, the annual displays wax and wane in splendor, there is no absolute cessation in the occurrence of star-falls on the date corresponding to such a system. And taking full account even of the marked diminution which actually occurs, we are yet compelled to assign an enormous longitudinal extent to that portion of the system which has been poetically termed "the gem of the meteor-ring." For example, in the November meteor-system, this portion of the ring cannot be less than 1,000,000,000 of miles in length. As to the width of a meteor-system — that is, its extent in a direction measured in the plane of its orbit — we have no satisfactory information, because a meteor-system may extend enormously on either side of the point through which the earth's orbit intersects it, and yet no trace of that extension be recognized by observers on the earth. Still we may conclude that this dimension lies in extent somewhere between the longitudinal extension of the system and the depth of the meteor-zone — that is, the length of a line taken through its square to the plane in which it lies. Now, of this last dimension we can form a tolerably accurate estimate in many instances. We know that so long as meteors belonging to any system are flashing into view, our earth is still plunging through the system; and if we know the position of the system we can determine its depth in this way, just as we could determine the breadth of a range of hills if we noticed how long a train, travelling with known velocity, took in passing through a tunnel which traversed the range of hills in a known direction. Judged in this way, the depth of the November meteor-zone would seem to be one hundred thousand miles in the part traversed by the earth in 1866, about sixty thousand miles in the part traversed in 1867, and considerably greater (though the zone was more sparsely strewn with meteors) where the earth crossed the system in 1868 and 1869.

Now, as regards the density with which meteors are strewn in any known system, I must remark on a mistake which has been very commonly made. It has been thought necessary to consider the velocity with which the meteors themselves travel as well as the earth's velocity, in order to determine, from the average interval of time separating the appearance of successive meteors, the average distance separating neighboring meteors from each other. This, however, is an erroneous mode of dealing with the problem. We need only consider the earth's velocity, since the meteoric motions cannot possibly tend to increase the total number of encounters.* Let us apply this consideration to enable us to form a rough estimate of the number of bodies in the richer part of the November meteor-system. We may fairly assume that, taking the average of the four displays of the years 1866-69, the earth encountered more than one meteor per minute as she swept successively through the system; or, conveniently for our purpose, that an average distance of 1,000 miles separates meteor from meteor throughout the "gem of the ring." Now, the length of the great cluster is at least 1,000,000,000 miles, its thickness may be fairly assumed as averaging 100,000 miles, and its width can hardly be less than ten times its thickness, since the forces acting on the system tend much more largely to affect its width than its thickness. Thus, with the assumed average of distance (1,000 miles), we find that the cluster cannot contain less than (1,000,000 × 100 × 1,000) or one hundred thousand million members.

Mr. Alexander Herschel, from observations of the amount of light given out by these bodies, and a calculation founded on the velocity with which they penetrate our atmosphere, has come to the conclusion that they must, for the most part, be very small, rarely, perhaps, exceeding a few ounces in weight. We shall certainly not exaggerate their weight if we assign one-hundredth part of an ounce to each. We thus obtain for the weight of the whole cluster one thousand millions of ounces, or about twenty-eight thousand tons. The actual weight of the

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* Obviously the total number of meteors encountered during the earth's passage through a meteor-stream will be the number contained in a cylindrical space having a cross-section equal to the earth's, and traversing the meteor-stream from side to side. The motion of the meteors will affect the particular set of meteors actually found within this space as the earth traverses it, but will not affect their number, assuming a general uniformity of meteoric distribution.

November meteor-system cannot, however, but enormously exceed this amount; and therefore we recognize how erroneous that opinion is which an eminent astronomer recently expressed, who asserted that the united weight of all the bodies other than planets in the solar system must be estimated rather by pounds than by tons. We have certainly no reason for thinking that the November system, though one of the most important encountered by the earth, is exceptionally important in the solar system. On the contrary, we have every reason the laws of probability can afford us, for believing that there must be millions of systems equally or more extensive. And, further, the fall of enormous masses, many tons sometimes in weight, upon the earth, would point to the conclusion that the members of the November system are exceptionally insignificant as regards their individual dimensions. So that we seem forced to the conclusion that the aggregate weight of the various meteoric systems circulating around the sun must be estimated by billions of tons rather than by any of our ordinary units.

I have already referred to the relation which has been detected between comets and meteor-systems. *Bizarre* as the relation appears, it has been established on evidence which cannot reasonably be disputed. It carries with it results of extreme interest and importance.

I do not propose here to enter into any consideration of those enormously difficult questions which are suggested by the study of cometic phenomena. That they will before very long receive their solution I confidently believe; but in the present state of our knowledge it would indeed be hazardous to speculate as to what that solution may be. I may remark in passing, that, while I recognize in Dr. Tyndall's recently-promulgated theory on the subject the indication of a highly-suggestive and promising line of research, I cannot but feel that cometic phenomena are far too complicated to be directly accounted for in the way pointed out by that distinguished physicist. Some of the more obvious, and, I may add, the more generally known phenomena, do indeed appear to receive a solution when examined under the light of Dr. Tyndall's researches, but numbers of others remain not only unaccounted for, but standing apparently altogether opposed to his theory.*

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* The theory recently put forward by Prof. Tait is altogether incon-

But for my present purpose the facts to be principally noticed are in a sense independent of any theory which may be formed respecting the nature of comets. We know that the dimensions of these objects are in many cases enormous. We know, further, that there must be many thousands of comets remaining undiscovered for each that our astronomers have detected. And, lastly, we are led to recognize the observed association between certain meteor-systems and certain comets as indicative of a general law by which, in some way as yet unexplained, comets and meteors are associated together. Thus, independently of the considerations already adduced, we are led to the conclusion that meteor-systems must be very numerous; while from the fact that a meteor-system so important as the November stream is associated with a comet so insignificant as Tempel's, we conclude that those magnificent comets which have blazed in our skies — a source at once of wonder and perplexity to the astronomer — must be associated with systems of bodies incalculably more important than the meteor-system which has so often filled the heavens with falling stars.

Now, combining all these results, we seem fairly led to the conclusion that purposes of the utmost importance in the economy of the solar system must be subserved by these uncounted thousands of meteoric streams. If, indeed, we could suppose that the planets steered clear of them, and that the bodies composing them simply circulated unceasingly in their orbits, we might form another opinion. But we know that meteors are continually falling upon the atmosphere of our own earth, either there to be dissipated into finest dust or to pass onward, with or without explosion, to the actual surface of the earth; and we cannot doubt that in a similar way countless thousands of meteors are falling, not only upon all the primary members of the solar system, but upon asteroids and satellites — nay, are even streaming in among the minute bodies composing the rings of Saturn. These encounters cannot be wholly without result, and it is quite conceivable that most injurious consequences might ensue to the inhabitants of all the worlds in

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sistent with the history of many comets. Indeed, I have been unable to find a single comet whose recorded changes of appearance countenance Prof. Tait's views.

the solar system if the continual supply of meteoric matter were importantly diminished.

Now, if meteoric masses fall continually upon the planets, such masses must fall in numbers inconceivably greater upon the sun; and it is here, unless I mistake, that the great purpose of the meteoric systems becomes apparent.

Let us clearly recognize, however, why and how the sun must be assaulted by a continual inrush of meteoric bodies. We have seen how enormous must be the number of these bodies; we know how swiftly they travel, and on what eccentric orbits; but we must go farther before we can prove that they fall upon the sun. For example, the November meteors are enormous in number, and travel with enormous velocity in a very eccentric orbit, but they do not approach the sun within a distance of nearly ninety millions of miles. Nor, indeed, can any known meteoric system pour a steady hail of meteors, so to speak, upon the sun; for he is the ruling centre of every meteoric system, and therefore under ordinary circumstances the meteoric orbits must pass around him, and not in such a direction as to intersect his substance.

But it is to be remembered that meteors must be infinitely more crowded in the neighborhood of the sun than at a distance from him. An indefinitely large number of meteoric orbits must absolutely intersect in the immediate neighborhood of the sun; and collisions must continually be taking place as countless thousands of meteoric flights rush toward and past and then away from their perihelia. Where these perihelia lie close to the sun, the velocity with which the meteors travel must exceed two hundred miles per second, and therefore the collision even of two minute meteors must result in the generation of an enormous amount of light and heat. But that is not all. Among the collisions thus continually taking place in the sun's neighborhood there must be a considerable proportion in which the two bodies are brought momentarily almost to rest by the shock. In such cases the combined mass of the two meteors would fall directly upon the sun, a fresh supply of light and heat being generated as they were brought again to rest upon his surface.

Whether in the continual collisions of meteors among themselves, and in their precipitation upon the sun's surface, we have a sufficient explanation of the seemingly exhaustless emission of light and heat from the sun, I should not care positively to assert. Prof. Thompson, who was one of the first to adopt this view, has, I believe, abandoned it; though it is worthy of remark that the strongest evidence in its favor has been obtained since he withdrew his support from it, or at least admitted that the downfall of meteors on the sun's surface is not *alone* sufficient to account for the solar light and heat. But I am quite certain that there is no flaw in the evidence I have adduced from the laws of probability; and that we are bound to accept, as a legitimate conclusion from that evidence, the theory that at least an important proportion of the sun's heat is supplied from the meteoric streams which circulate in countless millions around him. I believe that, without adopting any unreasonable assumptions, it might readily be shown that the whole even of that enormous supply of light and heat which the sun emits on every side is derived from the meteoric streams belonging to the solar system or drawn in from surrounding space, as the sun, attended by his family of planets, sweeps onward amid the stellar groups.

If this view be correct, then the meteor-systems constitute, indeed, a most important part of the sun's domain. They may be said almost to share with the sun a title to be regarded as the source of all the forms of force which exist throughout the solar system. If, in the energies of living creatures on earth, in the forces derived from the fuel that propels our engines, or in the power of winds and storms, we trace the action of the ruling centre of the solar system, we may trace back the chain of causation yet one link farther, and see in the sun's emission of light and heat the result of forces inherent in the meteoric systems which circle around him.

But we must not forget one most important consideration, which makes the sun (as might be anticipated) again the chief source of all the forms of force existing within his system. The motions of the meteoric masses are almost wholly due to the sun's attraction; and therefore, in so far as those motions are to be regarded as a means of renewing the solar heat, we must regard the sun's attractive energy as the source whence his heat and all the other forms of force which he exerts are in reality derived.

Yet one step farther. The sun's attractive energies might be increased a thousand-fold, and yet not avail to supply the various forms of force which are required by his dependent worlds, were there no external material on which those energies could act in such sort as to lead to the continual inrush of matter upon the solar surface. Nor would it suffice if such materials, even in enormous quantities, existed *close* to the sun. It is the distance from which that material is dragged toward the sun which gives that orb the power of imparting those tremendous velocities to which the collisions of the meteoric bodies owe their real effectiveness. We thus find in *distance,* in the simple element of *scale,* the true source of the various forms of force which are continually exerted throughout the solar system. The sun surrounded by millions on millions of meteoric masses close at hand would be powerless, but placed as ruler over a space far wider than the sphere circled by Neptune's orbit, amid which space those countless millions of meteors are distributed, he becomes forthwith the centre of a thousand forms of force, gathered by him continually from the systems of meteors circling around him, and distributed by him abundantly and without ceasing to his dependent worlds.*

It will not fail to be noticed by the thoughtful reader that, adopting this view of the relation in which meteoric and cometic systems stand with respect to the sun, it seems necessary that we should regard those planets which I have endeavored to raise to the dignity of secondary suns, as subordinate centres of attraction, around which countless thousands of meteoric systems may be supposed to circle. Have we any evidence pointing to such a conclusion?

Now, there can be no doubt that if Jupiter, the nearest of these secondary suns, did so act upon a passing comet as to compel that body to circle in future around *him,* instead of pursuing its course around the sun, we could not in any way become conscious of the event unless the comet were an exceptionally large one. I conceive, however, that such an event, though undoubtedly possible,* must be so uncommon, that the number of cometic systems thus forced to own Jupiter as their centre of attraction must be relatively few. But in another way the planet does exhibit his power as a comet-ruler, making comets recognize him as a sort of subordinate master, the sun being their primary ruler. When comets coming from outer space pass near enough to Jupiter, he sways them so markedly from the orbit they are pursuing that the scene of encounter becomes the aphelion of their orbit, or nearly so. Thence they pass on their new orbit to their perihelion, returning again presently to the scene of their encounter with Jupiter, and so revolving in an orbit having its aphelion close by the orbit of Jupiter, until haply the giant is again near the scene of encounter at the moment when the comet comes back to it. In this case a fresh struggle takes place, the overmastering attraction of the planet necessarily prevailing, and the comet being often dismissed on a new orbit, whose perihelion, instead of its aphelion, lies close by the orbit of Jupiter.

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* Just as this work was about to be placed in the printer's hands I received from Prof. Kirkwood, of America, one of his valuable contributions to the history of the solar system. In it he points to the evidence we have that the sun, as he speeds onward through space, passes through regions in which cometic and meteoric materials are now richly, now sparsely strewn, and gathers in accordingly new stores of force of greater or less amount. The bearing of the views of this acute and soundly-reasoning astronomer (the Kepler of our day), not only on the theories dealt with in the above chapter, but on those considered in the chapters which follow, will be seen at once.

Now, we know that such events as these must be of frequent occurrence as Jupiter sweeps swiftly round on his orbit. For we recognize several comets which have evidently been compelled by Jupiter to take up such orbits as I have spoken of — a family of comets, in fact, including Encke's, Faye's, and Brorsen's comets, Winnecke's short-period comet, and several others. We judge further, from the laws of probability, that, for each discovered comet of this family, there must be thousands which have escaped detection. So that around the orbit of Jupiter (if not around Jupiter himself) there cling the aphelia of myriads of cometic orbits, whose perihelia lie at all conceivable distances from the sun less than the distance of Jupiter.

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* It is necessarily possible in the case of any planet, but must in many cases be highly improbable. For example, astronomers sometimes assert that meteoric masses passing near the earth might become satellites of hers, but in reality this is a very unlikely event, because the maximum velocity which a body traveling under the earth's influence can have (that is, the velocity acquired by a body travelling from infinity to a perigee close to the earth) is less than the velocity with which a body circling on any orbit round the sun would move when at the earth's distance from him, unless its orbit were very eccentric and the aphelion close by the earth's orbit. Bodies travelling from outer space toward the sun cannot by any possibility become satellites of the earth, because they would always have a velocity greater than that which her attraction can master. Even in the rare event of their grazing her atmosphere, and so losing a large share of their velocity, they could not become permanent satellites of hers, because, returning to the scene of encounter, they would lose yet a larger share of their velocity, and so must be brought, and that soon, to her surface.

Saturn also has his family of comets; so also have Uranus and Neptune. The comet associated with the November meteors belongs indeed to the Uranian comet-family, and the epoch (126 A.D.) has even been pointed out when this comet fell under the dominion (subject always to the sun's superior control) of that distant planet.

And here I may refer to a view which I have long entertained respecting the purposes which meteoric and cometic systems have fulfilled in the past history of the solar system.* We know that the materials composing meteors, and we conclude therefore, that those composing comets, do not differ from those which constitute the earth and sun, and presumably the planets also. Therefore, under the continual rain of meteoric matter, it may be said that the earth, sun, and planets, are *growing*. Now, the idea obviously suggests itself, that the whole growth of the solar system, from its primal condition to its present state, may have been due to processes resembling those which we now see taking place within its bounds. It is of course obvious that, if this be so, the number of meteoric and cometic systems must have been enormously greater originally than it is at present. Countless millions of meteoric systems, travelling in orbits of every degree of eccentricity and inclination, travelling also in all conceivable directions around the centre of gravity of the whole, would go to the making up of each individual planet. A marked tendency to aggregate around one definite plane, and to move in directions which, referred to that plane, corresponded to the present direction of planetary motion, would suffice to account for the present state of things. The effect of multiplied collisions would necessarily be to eliminate orbits of exaggerated eccentricity, and to form systems travelling nearly on the mean plane of the aggregate motions, and with a direct motion. Further, where collisions were most numerous, there would be found not only the most circular resulting orbits, not only the greatest approach to exact coincidence of such orbits with the mean plane of the whole system, but the bodies formed out of the resulting systems would there exhibit rotations coinciding most nearly with the mean plane of the entire system.*

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* Since the present chapter was written, I find that the hypothesis here put forward has in a general way been touched on by more than one astronomer and physicist. I believe, however, that here, for the first time, it has been associated with the chief features of the solar system. It was suggested in note B (Appendix) to my treatise on Saturn. But, as a matter of fact, when that note was written, as also when those passages were published in which the same hypothesis is touched by other authors, the decisive evidences in favor of the theory were wanting.

It seems to me that, not only has this general view of the mode in which our system has reached its present state a greater support from what is now actually going on than the nebular hypothesis of Laplace, but that it serves to account in a far more satisfactory manner for the principal peculiarities of the solar system. I might indeed go further, and say that, where these peculiarities seem to oppose themselves to Laplace's theory, they give support to that which I have put forward.

For example, what is there in the nebular hypothesis which affords even a general explanation of the strange varieties of size observed in the planetary system? How can that hypothesis be reconciled with the remarkable variations of inclination observed among the planets, or with the retrograde and almost perpendicular motion of the satellites of Uranus? Nor, again, is the hypothesis consistent with the observed peculiarities of motion of those meteoric systems which we must now regard as regular members of the solar system.

Now, according to the hypothesis I have put forward above, a general explanation of all these matters is at once suggested. Let us consider:

In the neighborhood of the great central aggregation which would undoubtedly result from the motions of such meteoric systems as I have considered, all the motions would be very rapid. They would, in fact, resemble the motions now actually observed in the sun's neighborhood. Here, therefore, subordinate aggregations could form with difficulty, since they would have small power of overruling meteoric systems rushing with so great a velocity past them. In the sun's immediate neighborhood, then, we should expect to find relatively small planets;

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* This conclusion depends on a well-known law of probability. It may be thus illustrated: If we have in a bag a hundred white and a hundred black balls, and take out at random a number of balls, then the larger that number, the more nearly (in all probability) will the number of black and white balls included in it approach to a ratio of equality.

and we do accordingly find that Mercury, nearest to him, is the smallest of the planets, Venus larger, and the earth (yet farther away) not only larger than Venus, but adorned with an attendant satellite.

Now, at a much greater distance from the sun the meteoric motions would be so much less, that here, supposing only a suitable mean density of aggregation, it would be possible for subordinate centres of aggregation of far greater magnitude to form. These centres would increase in importance as they swept round the central aggregation, continually gathering fresh recruits. Indeed, though, *as now*, they would not be able to prevent the major part of the materials rushing from outer space toward the sun from aggregating round *him*, they would still gather in no inconsiderable portion of those materials. Where the largest portion would be gathered would depend on the way in which (taking a general view of the system) the quantity of material increased toward the neighborhood of the centre. For clearly, while distance from the sun would increase the facility with which materials would be gathered in — since the sun's influence would diminish with distance, it would also affect the quantity of material available — since, from a very early period, the system must have begun to show an appearance resembling that now presented by the zodiacal light, that is, a general increase of density toward the centre.

Assuming that the region of maximum aggregation was that where the influence of the ruling centre first became so far diminished with distance as to render the formation of a great subordinate aggregation possible, we should have the innermost of the outer series of planets also the most bulky; and next, within that giant planet we should find a relatively barren space, cleared of material not only by the sun's still powerful influence, but also by the influence of this first important subordinate aggregation. The initial assumption is, in itself, at least not improbable, and, having once admitted it, we find an explanation of the giant mass of Jupiter, of the comparative poverty of material just within the orbit of Jupiter, and hence, of the condition of the asteroidal zone, and of the smallness of the planet Mars next within that zone — though this planet far outweighs (according to Leverrier's estimate) the united mass of all the asteroids. Beyond the orbit of Jupiter, we should expect (after passing an enormously wide space, bare of worlds) to find still a great abundance of material, and an even greater facility in the aggregation of that material. Thus the existence of the planet Saturn, next in importance to Jupiter, and surpassing him in the complexity of his attendant system, is accounted for; yet farther away we look for and find still an abundance of material, and that material somewhat more uniformly strewn, while the sun's small influence is indicated by the existence of satellites, of which doubtless many more will one day be discovered by astronomers.

And as to the rotations of the various members of the solar system we find some account, necessarily not exact, given by this theory. I have mentioned above the results to be looked for; those observed are closely accordant with that view. Thus the sun, the largest member of the system, and specially preeminent within its inner division, rotates on an axis inclined but about seven degrees to the mean plane of the system. Mars, the least member of this system, has an inclination of no less than twenty-eight degrees; the larger earth an inclination of but twenty-three degrees. The inclinations of Venus and Mercury are undetermined; they may be expected to be large, not merely on account of the smallness of these bodies, but on account of their proximity to the sun. Of the outer division of the system, Jupiter, the largest, has an inclination of little more than three degrees; Saturn has a very considerable inclination (more than twenty-six degrees); Uranus has an inclination which may be described as actually greater than ninety degrees, since he rotates *backward* with his equator inclined seventy-six degrees to the ecliptic. And lastly, if the observations hitherto made on Neptune's satellites are to be trusted, this planet, probably, rotates in a retrograde manner, his equator being inclined some twenty-six degrees to the horizon; so that, to render the comparison between his rotation and that of the other members of the solar system complete, he may be said to rotate in a direct manner with his equator inclined some one hundred and fifty-four degrees to the ecliptic.

The great inclination and eccentricity of many of the asteroidal orbits are also accounted for more satisfactorily by this theory than by the nebular hypothesis. In fact, there is an absolute incorrectness in the assertion that the smallness of the asteroids can (on the ordinary view of their origin) explain the relatively irregular nature of their motions. Their minuteness doubtless brings them more under the disturbing influence of Jupiter than a single massive planet at the same distance from the sun would be. But the attraction of Jupiter can have no influence in causing the asteroids to depart so widely as they do from the ecliptic, since his path lies quite close to the ecliptic, and even nearer to the mean plane of the solar system. But bodies formed as the asteriods are supposed to be, according to the hypothesis I have suggested, would necessarily exhibit a much greater variety of motion than would be recognized in the case of the larger planets.

Another point in which, as I conceive, my hypothesis is more satisfactory than the nebular one, consists in the fact that it suggests an explanation of the peculiarities observed in the planetary periods. Prof. Kirkwood's researches into the various relations of commensurability presented among the periods of planets and satellites, and the known effects of commensurability in encouraging the accumulation of planetary perturbations, will at once suggest to the mathematical reader the way in which a system, forming in such a manner as I have imagined, might be expected to exhibit the presence of law as regards distances and periods. I know of nothing in the nebular hypothesis which encourages the belief that a system framed as Laplace conceived the solar system to be, would exhibit any such laws as are found within the planetary scheme.

The hypothesis I have put forward also gets rid of that which has always seemed to me the great difficulty of the nebular hypothesis. According to the views of Laplace, Neptune must have been formed millions of ages before Uranus, Uranus as long before Saturn, Saturn as long before Jupiter, and so on. Now, we know that the appearance of those primary members of the solar system which we are best able to study does not indicate any such enormous disproportion in the ages of the planets, even if it does not indicate that the planets were formed nearly at the same era. According to my hypothesis, the various processes of aggregation would go on simultaneously (just as the influences which Jupiter has on comets are *now* exerted simultaneously with those more powerful influences possessed by the sun); and though the various orbs formed by those processes would not necessarily be completed simultaneously, there would be no such enormous disproportion in their age as is necessary according to the theory of Laplace.

Yet another strong point in favor of this hypothesis resides in the circumstance that we now have every reason to believe that all the planets are constituted of the same elements. When it was thought that Jupiter might be a watery globe, for instance, there was some evidence in favor of Laplace's theory. But we now know that Jupiter is not constituted differently, in all probability, from the earth and sun, as according to Laplace's theory he must have been. Since, then, we know that meteors contain the same elements which exist in the constitution of sun and planets, we have here a very strong argument in favor of the view that they have played the important part I have assigned to them in the formation of the solar system.

But, after all, the strongest evidence in favor of the hypothesis I have suggested, consists in the fact that the processes by means of which I conceive the solar system to have been formed, are undoubtedly going on before our eyes. There may be little, indeed, in the downfall of meteoric showers to suggest the idea of world-formation or sun-formation; little in the present aspect of the zodiacal light or of the solar corona, to present to the mind's eye a picture of that vaster agglomeration of meteoric and cometic systems, all speeding with inconceivable velocities on their interlacing orbits, which I imagine to have been the embryon of the solar scheme. But sun and planets *are* growing, however slowly, as the meteoric hail falls continuously upon them; the zodiacal light and the solar corona *are* doubtless due to the existence of meteoric systems, resembling (however relatively insignificant) those which I have pictured as the materials of the planetary scheme. In the Saturnian rings, also, which have been proved by the researches of Maxwell and others to consist of multitudes of discrete bodies, we have evidence of the same sort in the case of a subordinate centre of aggregation. So that we have a form of evidence which was wanting in the case of the nebular hypothesis, in favor of this other hypothesis, by which, as in Laplace's, the present state of the solar system is regarded as the result of a process of development, and not of special creative fiats of the Almighty.

In this last respect, the hypothesis I have put forward will doubtless seem objectionable to those who imagine that, in indicating processes according to which the solar system may have reached its present condition, astronomers are attacking the attributes of God. This will be the more unfortunate, because those who entertain this strange view may be regarded as probably so far beyond the reach of argument as to be unlikely ever to abandon their objection. Otherwise, it might avail to point out that, as, in all that surrounds us, we find God acting through second causes, we can have no reason for assigning limits to the range of space or time within which He *so* acts; that is, we can have no reason for believing that we can point to a time when He acted *directly* upon the universe: and further, that it gives an altogether higher idea of that wisdom which must, in any case, be far above our conceptions, to regard the laws of God as so perfect that they operate always to work out His will — without the necessity of special interference on His part — than to see His hand directly operative in all the phenomena of the universe.

CHAPTER X.

OTHER SUNS THAN OURS.

WE ARE now to venture into regions where we shall no longer have clear lights to guide us. Tremendous as are the dimensions of the solar system, the widest sweep of the planetary orbits sinks into insignificance compared with the distances which separate from us even the nearest of the fixed stars. From beyond depths which the human mind is utterly unable to conceive there come to us the rays of light which myriads of those orbs are pouring forth, and it is from the lessons taught us by these light-rays that we are to form our ideas concerning the nature of the orbs which emit them. Very carefully and cautiously must we proceed, if we would avoid being led into vain imaginings. It will but mislead us to pass a single step beyond the path which is dimly lighted for us, and yet that path is so narrow and so obstructed with difficulties, that we find ourselves continually tempted to leave it, and to venture forward on the alluring and easy paths which speculation opens out on every hand around us.

And yet we may well remain content to listen only to the teachings of known facts. Even so restraining ourselves, we have in reality a wide and noble domain to explore. Facts which seem severally unimportant, are found, when considered as parts of a grand whole, to indicate relations so impressive and so interesting, that the revelations of the telescope within the solar system are apt to seem commonplace beside them. We have, in fact, to consider no longer the structure of a system — the architecture of the universe is our theme.

Let us examine carefully the evidence which science has gathered together for us, endeavoring at each step to gain the full amount of knowledge the several facts involve, while, at the same time, cautiously refraining from any attempt to overstep the bounds indicated by our evidence.

In the first place, let us consider what may be learned from the analogy of the solar system. The study is an inviting one, since the discoveries on which we are to found our views have been made so recently, that the subject has all the charm of novelty and freshness, while it involves the consideration of the soundest and most instructive mode of pursuing our researches.

We have seen in the solar system a variety and complexity of structure, such as, half a century ago, few astronomers would have thought of ascribing to it. When Sir William Herschel began that noble series of researches amid the sidereal depths by which his name has been rendered illustrious, he saw in the solar system a scheme very different indeed from that which is presented to our contemplation. He beheld a vast central body, surrounded by a limited number of orbs, some of which were the centres of subordinate schemes of greater or less extent. When we have added the ring of Saturn as the only formation differing from planets and satellites in character, and the comets few and far between, which seemed rather accidental tributaries of the sun than regular members of his family, we have considered all the features which the solar system, as known in Sir William Herschel's day, presented to the contemplation of astronomers.

With us it is very different. We seen that there exists within the solar system a variety of size and structure, of motion, arrangement, and aggregation, which is already inconceivable, and yet doubtless but faintly shadows forth the real complexity and richness of the scheme swayed by our sun. Perhaps it is in considering the solar system in the particular light in which, in this treatise, I have had occasion to present it, that this wonderful variety of conformation is made most strikingly apparent. But, apart from all speculative theories, there can be no doubt that the solar system presents to us a subject of study amazing in itself, but most amazing when we regard it as supplying the analogies which are to guide us in forming our views respecting the sidereal system. Besides the family of planets circling round the sun, besides the system of dependent orbs which circle round the planets, we see a zone in which independent planets circle by hundreds, perhaps even by myriads, around the solar orb; we see the ring of Saturn composed of thousands of tiny bodies; we see the meteoric systems in countless hosts; we see the comets of our scheme in millions on millions; and less certainly, but still not indistinctly, we recognize the existence of a multitude of new and hitherto unsuspected forms of matter within the circle of our sun's attraction.

What opinion, then, are we to form — even here, at the very outset of our inquiry — respecting the sidereal scheme of which our sun forms but a unit? Surely it would be to lose sight of the significant lesson taught us by the solar system, it would be to forget how sure and safe a guide the greatest of modern astronomers found in the teachings of analogy, to adopt the same view now which that great astronomer adopted a century ago. If, viewing the solar system as consisting of discrete orbs, comparable one with another in size, and distributed not without a certain uniformity around their ruling centre, Sir William Herschel held that the sidereal scheme presented somewhat similar relations, surely *we,* who know certainly that the solar system is constituted so differently, must adopt a far different view of the sidereal scheme also.

Let us remember that there is here — so far as our respect and admiration for Sir William Herschel are concerned — a choice between two courses. Assuming, as indeed is just, that the views of our great men are not rashly to be thrown on one side, we have to choose whether we would rather abandon the views which Sir William Herschel formed about *facts,* or the views which he formed about *principles.* If we accept his opinion (or rather, after all, his mere suggestion) that the stars are tolerably uniform in magnitude and distribution, we must abandon the analogy of the solar system. If, on the contrary, we accept Sir William Herschel's often-expressed opinion that, in theorizing about the unknown, there can be no safer guide than the analogy of known facts, we must abandon the view (which seemed to him but probable) that the stars are distributed with tolerable uniformity throughout our galaxy, and are comparable *inter se* in magnitude and splendor.

There can be no doubt which course is preferable. We know certainly that Sir William Herschel was often mistaken, as all men must be, in matters of fact; while we know with equal certainty that he owed the marvellous success with which he theorized, to his adoption of the principle that analogy is the chief and the best guide for the student of astronomy.

We are compelled, then, in our very respect and admiration for the greatest astronomer of modern times, to regard the constitution of the sidereal system as, in all probability, very different from what he imagined.

We must be prepared to expect an infinite variety of figure, of structure, of motion, and of aggregation throughout the galactic scheme. If some orbs within that scheme seem probably to be suns like our own, we must not be surprised to find others which are probably far larger or far smaller. We may look for objects differing as much from the suns of the sidereal system as the asteroidal zone differs from Saturn or from Jupiter. So that, if we should recognize evidence of the existence of clusters of minute stars — a whole cluster, perhaps, not equalling in real importance the least of the suns of the system — we may accept that evidence without any scruples suggested by the improbability of the conclusion to which it points. Again, we may expect to find schemes within the sidereal system, differing as much from discrete stars or star-clusters as the rings of Saturn differ from the primary planets or from the asteroidal zone. So that, if we should recognize evidence of the existence of relatively minute clusters, whose components are either so small or so closely aggregated as not to be separately visible even in our most powerful telescopes, this evidence may fairly be accepted as accordant with the only analogy we have for our guidance. Yet once more: we may look for systems differing as much from all ordinary star-clusters as the eccentric and far-reaching meteor-systems differ from the symmetrical rings of Saturn. So that, if we should find evidence of strange schemes within the sidereal system, schemes presenting every *bizarre* variety of figure, with strange complexities of spiral whorls or outlying branches, losing themselves, as it were, in the depths toward which they seem to extend — this also need not surprise us: we need not conclude that *here*, at any rate, we are looking beyond the bounds of the sidereal system, and gazing upon external galaxies, for the analogy we have chosen for our guidance teaches us that such structures were to be expected within the scheme of which our sun is a component. And, finally, if we should find reason to assure ourselves that there are objects in the depths of space whose very substance and constitution are different from those of all other objects within the sidereal system, we need by no means believe that the objects thus singularly constituted belong to, or form, external systems. For the millions on millions of comets which form part and parcel of the solar system present a precisely analogous difference of structure, as compared with the other members of that system.

Having thus replaced the erroneous analogies to which — through no fault of his own — Sir William Herschel was led to look for guidance, by the more trustworthy analogies which the recent progress of astronomy has afforded for our instruction, we may proceed to consider the direct evidence we have respecting the constitution of our galaxy.

In the first place, let us examine the evidence which points to the dimensions of the sidereal system.

That the nearest members of the system lie at enormous distances from us is proved by the fact that, as the earth sweeps on her vast orbit round the sun, no appreciable change is observed in the configuration of the star-groups. That a circle having a diameter of more than one hundred and eighty millions of miles should be swept out year by year as the earth traverses her orbit, and yet that the surrounding stars should exhibit no change of place, is at once the most striking and the simplest evidence we have, of the enormous scale on which the sidereal system is constructed. And yet this first obvious fact sinks almost into insignificance when we regard thoughtfully the teaching of modern instrumental astronomy. There might be a real shifting of apparent position which yet the unaided eye would fail to detect, and such a change would indicate distances so enormous that the mind fails altogether to conceive their real significance. But the exact instruments of modern times would exhibit a change of place infinitely more minute than any which the unaided eye could recognize. If a star shifted by so much as the ten thousandth part of the moon's apparent diameter, modern astronomers could assure themselves of the change of place. And when we remember that in precisely the same proportion that we increase the exactitude of instrumental observation we increase also the significance of the stars' apparent fixity of position, it will be seen at once how astounding is the lesson conveyed by the fact that all but a very few indeed of the stars remain absolutely unaffected — even under the most powerful instrumental examination — by the enormous range of the earth's orbital motion.

We can roughly estimate the distances of the few stars which are thus affected, and thence — on the hypothesis that the intrinsic brilliancy of their light is the same as the sun's — we may form some idea of their dimensions. I shall, however, only apply this process, in detail, to a single case, because my present object is rather to indicate in a general way the scale on which the sidereal system is constructed, than to enter at length on the more exact details which find their place in ordinary treatises on astronomy.

The star Alpha Centauri is one of the brightest in the heavens, Sirius and Canopus alone surpassing it in splendor. But it is not its exceptional brilliancy alone which led astronomers to regard it as likely to afford evidence of an apparent change of place corresponding to the earth's real change of place as she sweeps round her orbit. Of course, the brightest stars are presumably. the nearest; but there is another indication of proximity at least equally important. The so-called fixed stars are in reality slowly moving onward on definite courses — slowly, that is, in appearance, though in reality their motions are doubtless inconceivably rapid. Now, these motions, the *proper motions* of the stars, as they are called, are as yet very little understood. We know only that the whole of the galactic system, is astir with life, but whither the orbs are severally tending we are not yet able to say. Nor do we know what portion of the stellar motions may be due to the undoubted proper motion of our own sun through space. This, however, may be regarded as certain, that, until we know something respecting the laws which regulate the stellar movements, we must regard the magnitude of a star's motion as probably an indication of relative proximity. Precisely as a man walking at a great distance from us appears to move much more slowly than one who is walking at the same rate close by, so the apparent rate of a star's motion is diminished in proportion to the star's distance from us. When, therefore, it was found that the star Alpha Centauri is moving more rapidly than other stars, this fact, combined with the great lustre of the star, led astronomers to suspect that it must be comparatively near to us.

Observations, made to determine whether the star shows any sign of an annual change of place corresponding to the earth's annual orbital motion, were rewarded by the detection of a very appreciable displacement. In fact, owing to the motion of the earth, each year, in a nearly circular orbit one hundred and eighty million miles in diameter, the star Alpha Centauri appears to trace out each year a minute oval path on the celestial sphere, the greater axis of the oval being equal in length to about $\frac{1}{900}$th part of the moon's apparent diameter.*

It follows from this that, in round numbers, the distance of Alpha Centauri from us is about twenty millions of millions of miles. The distance of the earth from the sun shrinks into insignificance beside this enormous gap. Even Neptune, though circling round the sun at a distance three hundred times vaster than that which separates us from that luminary, is yet relatively so much nearer than Alpha Centauri, that a sun filling the whole orbit of Neptune would appear, as seen from that star, but about one-ninth as large as the sun appears to us.

Now let us consider what dimensions we may assign to Alpha Centauri, on the assumption that the surface of this star emits a light as brilliant as that which proceeds from the photosphere of our own sun. We must not neglect the consideration that the star is double—the companion emitting perhaps about one-sixteenth as much light as the primary.† The distance of Alpha Centauri is equal to about two hundred and thirty thousand times that which separates us from the sun. Therefore, if removed to the star's distance, the sun would shine with only $\frac{1}{52,900,000,000}$th part of his present brilliancy. Now, according to the most careful estimates of the brilliancy of Alpha Centauri, the light we receive from that star is about $\frac{1}{16,950,000,000}$th of that we receive from the sun.‡ It follows, therefore, that the star emits about three times as much light as the sun; and therefore, so far as the emission of light is a criterion of size, the star may be regarded as considerably larger than our own sun. In fact, reducing the total light of the pair by one-sixteenth, we find that the primary must still emit about three times as much light as the sun, and therefore the diameter of the star, as thus estimated, would appear to exceed our sun's in the proportion of about seventeen to ten.

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* It hardly need be mentioned, perhaps, that this motion being superadded to the star's more considerable proper motion, the path which the star seems really to follow is a looped one, the size of each loop being small in comparison with the distance between successive loops.

† Sir John Herschel, observing the star with his twenty-feet reflector, thought the secondary brighter than it is usually considered. I cannot but think that, for a comparison of this sort, smaller telescopes may more safely be trusted.

‡ This estimate is founded on Sir John Herschel's comparison between the light of the star and that of the full moon, and Zöllner's comparison between the light of the full moon and that of the sun.

We have here, then, clear and decisive evidence in favor of the view that among the fixed stars there are orbs which may be regarded as veritable suns, worthy to be the ruling centres of schemes as noble as the solar system. For we know quite certainly that the greater number of the first-magnitude stars are very much farther from us than Alpha Centauri, with which, however, they are fairly comparable in brilliancy: so that they may be regarded as for the most part at least equal to that star in size and mass. Sirius and Canopus, indeed, must far surpass Alpha Centauri. The latter, though more than thrice as bright, exhibits no appreciable change of position as the earth circles round the sun. Sirius, which is more than four times brighter than Alpha Centauri, shows an annual change of position which certainly does not exceed one-fourth of that star's. It is therefore four times farther from us than Alpha Centauri, and, did it emit no greater amount of light, would appear to shine with but one-sixteenth of that star's lustre. As in reality it is four times as bright, the real amount of light it emits must exceed that of Alpha Centauri no less than sixty-four times, and that of our own sun no less than one hundred and ninety-two times. So that, judged from this indication alone, the diameter of Sirius may be held to exceed that of our sun in the proportion of about fourteen to one, an estimate which assigns to Sirius a diameter of nearly twelve million miles, and a volume two thousand six hundred and eighty-eight times as large as the sun's.

But, on the other hand, still confining our attention to this method of estimating magnitude, we find reason for believing that many of the visible stars must fall far short of our sun in magnitude. The sixth-magnitude double star, 61 Cygni, has been found to be nearer to us than Sirius, and about three times as far from us as Alpha Centauri. Now, we may assume that each component sends us about one-hundredth part of the light we receive from Alpha Centauri; it follows that the latter star, if removed to the distance at which 61 Cygni lies from us (when its light would of course be diminished to one-ninth of its present value), would outshine either component of that double star more than eleven times; hence (on the assumption that brightness is a fair measure of real dimensions), each component has a diameter less than one-third that of Alpha Centauri. We may roughly estimate the volume of each at about $\frac{1}{30}$th of that of the latter star. So that, remembering what has already been shown respecting the relation between Alpha Centauri and our sun, the two suns which form the double star 61 Cygni would each have a diameter equal to about $\frac{17}{30}$ths of the sun's, and a volume equal to about $\frac{2}{11}$ths. The sum of their volumes would be therefore about one-third of his; and it will presently appear that a perfectly distinct mode of estimation tends to show that the sum of their masses bears about the same proportion to the sun's mass.

But here at once we have evidence that there is a very wide range of magnitude among the fixed stars. We have seen reason to believe that Sirius is twenty-six hundred and eighty-eight times as large as the sun, while each of the suns forming the double star 61 Cygni would appear to have a volume less than one-fifth of our sun's, and therefore less than $\frac{1}{13,500}$th of the volume of Sirius. So that, by considering only three cases, we have found tolerably clear evidence of a range of variety in volume, reminding us forcibly of that which we recognize in the solar system. We cannot suppose that these three cases, which have been selected at random — so far as the question of volume is concerned — indicate any thing like the real limits within which the fixed stars differ in magnitude. So that we may confidently accept, as the most probable conclusion from the evidence before us, that the range of real magnitude among the fixed stars is very far greater than Sir W. Herschel was led to anticipate, when, nearly a century ago, he began his researches into the sidereal system.

But it is not sufficient that we should thus form an estimate of the nature of the fixed stars, from the amount of light they send to us. It is desirable — and fortunately it is practicable — to obtain information as to the absolute mass or weight of some of the fixed stars, and further to ascertain of what substances they may be composed, and in what condition those substances may exist. Mere lights, however glorious, or however wide the sphere within which they displayed their splendors, would not be fit to sway the motions of orbs resembling those which circle around our sun. Nor would such lights serve to indicate to the astronomer that, out yonder, myriads of millions of miles beyond the extreme limits of the solar system, there exist materials suited to form the substance of worlds resembling our own.

It seems a strange circumstance that astronomers should be able to form a more exact and trustworthy estimate of the weight of certain fixed stars than they can hope to form respecting the volume of any of those bodies. Let us consider what evidence we have on this point.

I have spoken of the star 61 Cygni as a double star. The smaller star shows very clear indications of orbital motion around its primary. That the two are associated together, and not merely seen, as it were by an accident, nearly in the same line of view, is indeed certain, because that peculiarly large proper motion already referred to is shared in by both. But many stars may be physically associated, and yet the distance really separating them may enormously exceed that by which they seem to be separated — since the line joining them is not necessarily square to the line of sight. The components of the star 61 Cygni have been carefully watched, however, and their motions show that they are circling around each other. The distance separating them is probably about half as large again as the distance of Neptune from the sun.

The period of revolution appears to be about five hundred and twenty years, which is more than three times as great as the period of Neptune. Now, we know that a planet placed at distance from the sun, equal to that which separates the components of 61 Cygni, would occupy a much less period than five hundred and twenty years in completing a revolution; in fact, its period would be about three hundred years. Hence it follows that the components of 61 Cygni are attracted together less forcibly than Neptune is attracted toward the sun, and therefore that the sum of their masses must be less than the sun's mass. It is easy to compute the actual proportion, and we find accordingly that the two components of 61 Cygni, taken together, weigh about one-third as much as our sun.*

The star Alpha Centauri is also a binary system, and though it has not been so systematically observed as 61 Cygni, some astronomers believe that its period has been even more satisfactorily determined. Indeed, there are peculiarities in the motion of 61 Cygni, which, without throwing doubt on the general conclusions deduced above, yet suggest that a third (probably opaque) orb affects the motions of the other two. From a careful comparison of all the observations made in recent times on Alpha Centauri, Mr. Hind has assigned to the components a period of revolution of about eighty-one years, and a mean distance of 13.6 seconds of arc, corresponding to a real distance exceeding the earth's distance from the sun some fifteen times. Since a planet placed at this distance from the sun would occupy less than sixty years in completing a revolution around that body, it follows that the mass of the two components of Alpha Centauri must be less than that of the sun. This result (if the data be considered trustworthy) would indicate a considerable difference between the condition of the star and that of our sun; for we have seen that the star gives out much more light than the sun. However, I believe that many years must elapse before we can regard the period of Alpha Centauri as satisfactorily determined.

Still, we have conclusive evidence in this case, as in that of the star 61 Cygni, that the component stars are really bodies of enormous weight, and consequently well fitted to sway the motions of families of planets. We conclude, therefore, that the fixed stars generally are *suns,* not mere *lights*; and, further, we are led to believe that there must be a general similarity in the conditions under which these bodies and our own sun emit light. And thus we are led to recognize other stars also — though as yet unweighed — as massive orbs, not merely supplying light to other worlds travelling around them, but regulating by their attractive influences the orbital motions of their dependent worlds.

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* It may easily be shown that, if a pair of bodies, circling around each other at a certain distance, take a certain time T in effecting a revolution, while another pair at the same distance take a time t, the former pair, taken together, have a weight which bears to the weight of the latter pair the ratio of t^2 to T^2.

But we owe to the revelations of the spectroscope the complete proof of these matters, besides evidence on other and equally interesting points.

It had long been known that the spectra of the fixed stars present a general resemblance to the solar spectrum, though of course very much fainter, and that dark lines can be seen in these spectra, some of which correspond with those in the sun's spectrum, while others seem to be new. So soon as the great discovery effected by Kirchhoff had been announced, it was seen at once that these dark lines in the stellar spectra afford the means of determining the constitution of the stars. It was only necessary that these lines should be identified by their correspondence with the lines belonging to known elements, in order to prove that these elements exist in the substance of the star. But, although the principle on which researches were to be conducted was sufficiently simple, many difficulties had to be encountered. Indeed, the attempts made by Airy, Secchi, and Rutherford, to solve the problem of determining the constitution of the stars by means of spectroscopic analysis, were unsuccessful; and it was not until Prof. Miller and Mr. Huggins commenced their famous series of researches that the problem can be said to have been fairly mastered.

Even in the hands of these eminent physicists the work was difficult, and its progress tedious. The weather necessary for the successful prosecution of so delicate a method of inquiry does not often prevail in our variable climate. The comparison between the dark lines in the stellar spectra and the bright lines belonging to various elements was not only a delicate and laborious task, but was singularly painful to the eyes. And other difficulties, into which I have not space to enter here, had to be encountered and overcome.

But, undeterred by these difficulties, the two physicists persevered in their researches, and were rewarded by results so interesting and important that their discovery may be said to constitute the most remarkable era in the history of sidereal research since the completion of the star-gaugings of the elder Herschel.

Two bright stars, Betelgeux, the leading brilliant of Orion, and Aldebaran, the chief star of Taurus, were examined with special care. Mr. Huggins remarks that the spectra of these stars are as rich in lines as the solar spectrum itself. The places of no less than eighty lines in the spectrum of Betelgeux were accurately measured, while as many as seventy lines had their places assigned to them in the spectrum of Aldebaran.

With respect to the former spectrum, Mr. Huggins remarks that it is most complex and remarkable. "Strong groups of lines are visible, especially in the red, the green, and the blue portions," a peculiarity, it may be remarked in passing, which serves to account for the well-marked orange-color of this star.

Now, here already we have very decided evidence as to the nature of the star; since the very fact that its spectrum presents the same general appearance as the solar spectrum, proves conclusively that the star is an incandescent body, whose light comes to us through certain vapors corresponding to those which surround the sun. Nor should we be able to regard the star as other than a sun, even though none of the elements known to us should appear to be present in its substance, or in the vapors surrounding it. For, clearly, we have no reason for believing that worlds can be formed out of those elements only with which we are acquainted, unless we find, as we proceed, that those elements actually do compose the suns which form the sidereal system. Of course, if this shall appear to be the case, our conclusions respecting the nature of the stars will be very much strengthened.

Now, when Prof. Miller and Mr. Huggins compared the lines in the spectrum of Betelgeux with the bright lines of certain terrestrial elements, they found that some of these elements do actually exist in the vaporous envelope of the stars. Thus, sodium, magnesium, calcium, iron, and bismuth, are present in Betelgeux. The lines of hydrogen, which are so well marked in the solar spectrum, are not seen in the spectrum of Betelgeux. We are not to conclude from this that hydrogen does not exist in the composition of the star. We know that certain parts of the solar disk, when examined with the spectroscope, do not at all times exhibit the hydrogen lines, or may even present them as bright instead of dark lines. It may well be that in Betelgeux hydrogen exists under such conditions that the amount of light it sends forth is nearly equivalent to the amount it absorbs, in which case its characteristic lines would not be easily discernible. In fact it is important to notice generally, that, while there can be no mistaking the positive evidence afforded by the spectroscope as to the existence of any element in sun or star, the negative evidence supplied by the absence of particular lines is not to be certainly relied upon.

In the case of Aldebaran the two physicists were able to establish the existence of sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury, in the vapors surrounding the star.

Besides these stars, fifty others were examined. The brilliant Sirius exhibits a spectrum of great beauty, though the low altitude which this star attains in our latitudes rendered the observation of the finer lines exceedingly difficult. But the two physicists were able to show that sodium, magnesium, hydrogen, and probably iron, exist in this gigantic sun.

All the stars examined exhibit spectra crossed by numerous lines; and, in a great number of the spectra, lines belonging to known terrestrial elements were detected.

And now let us consider the general bearing of these interesting discoveries.

In the first place, we are forced to recognize in the stars ***real suns,*** not mere lights. Doubtless Dr. Whewell did well in pointing out that astronomers had no right to regard the stars as suns, until they had some evidence that these orbs resemble the sun in other respects than in size, mass, or luminosity. And as in his day it appeared altogether unlikely that such evidence should be obtained, a real limit seemed placed to the speculations men might form as to the existence of other planetary systems besides those which circle around the sun.

But now we have precisely that evidence which Whewell required. We see that the stars are constituted in the same general way as the sun, and that, further, they even contain elements identical with those which exist in his substance. There is not indeed in every case, perhaps there may not be in any case, an exact identity of composition between star and sun, or between star and star. But this was no more to have been looked for than an exact identity of physical habitudes among the members of the solar system. That general resemblance of structure which indicates a general resemblance in the purposes which the celestial bodies are intended to subserve, is undoubtedly evident, when we compare the stars either with our sun or with each other.

I have already spoken of the conclusions to be drawn from the existence of the same materials in the substance of the sun that exist around us on this earth. I have shown that we are compelled to regard this general resemblance of structure as sufficient to prove that the other planets resemble the earth, since we have no reason to believe that our earth bears an exceptionally close resemblance to the sun as respects the elements of which she is composed.

Since, then, we have reason to believe that all the planets which circle around the sun are constituted of the same materials which exist in his substance, though these materials are not necessarily nor probably combined in the same proportions throughout the solar system, we have every reason which analogy can give us for believing that the planets circling around Betelgeux or Aldebaran are constituted of the same materials which exist in the substance of their central luminary.

Thus we are led to a number of interesting conclusions even respecting orbs which no telescope that man can construct is likely to reveal to his scrutiny. The existence of such elements as sodium or calcium in those other worlds suggests the probable existence of the familiar compounds of these metals — soda, salt, lime, and so on. Again, the existence of iron and other metals of the same class carries our minds to the various useful purposes which these metals are made to subserve on the earth. We are at once invited to recognize that the orbs circling around those distant suns are not meant merely to be the abode of life, but that intelligent creatures, capable of applying these metals to useful purposes, must exist in those worlds. We need not conclude, indeed, that at the present moment every one of those worlds is peopled with intelligent beings, because we have good reason for believing that throughout an enormous proportion of the time during which our earth has existed as a world no intelligent use has been made of the supplies of metal existing in her substance. But that at some time or other those worlds have been or will be the abode of intelligent creatures seems to be a conclusion very fairly deducible from what we now know of their probable structure.

But, secondly, apart from the information afforded by the spectroscope respecting the materials of which the stars are composed, the nature of the stellar spectra serves to prove most conclusively that the stars, besides supplying light to the worlds which circle around them, radiate heat also to them. Even if we were not certain that elements which are only vaporized at a very high temperature exist in the vaporous envelopes of the stars, yet the very nature of the light sent out by the stars indicates that these orbs are incandescent through intensity of heat. When we find that the spectrum of a planet's light resembles the solar spectrum, we do not indeed conclude that the planet is as intensely heated as the sun, because we know that the planets are not self-luminous. But, in the case of self-luminous bodies like the stars, we can conclude from the very nature of their spectra that these orbs are intensely heated. Of course we are rendered absolutely certain of this when we find that iron and other metals exist in the form of vapor in the stellar atmospheres.

The vast supplies of heat thus emitted by the stars not only suggest the conclusion that there must be worlds around these orbs for which those heat-supplies are intended, but point to the existence in those worlds of the various forms of force into which heat may be transmuted. We know that the sun's heat poured upon our earth is stored up in vegetable and animal forms of life; is present in all the phenomena of Nature — in winds, and clouds, and rain, in thunder and lightning, storm and hail; and that even the works of man are performed by virtue of the solar heat-supplies. Thus the fact, that the stars send forth heat to the worlds which circle around them, suggests at once the thought that on those worlds there must exist vegetable and animal forms of life; that natural phenomena, such as we are familiar with as due to the solar heat, must be produced in those worlds by the heat of their central sun; and that works such as those which man undertakes on earth — works in which intelligent creatures use Nature's powers to master Nature to their purposes — must go on in the worlds which circle around Aldebaran and Betelgeux, around Vega, Capella, and the blazing Sirius.

Recently it has even been found possible to render the stellar heat sensible to terrestrial observation, by methods which need not here be inquired into. Nay, the task of measuring the amount of heat received from certain stars has not been thought too difficult. Mr. Stone, making use of the powers of the great equatorial of the Greenwich Observatory, and ingeniously overcoming the numerous difficulties which exist in a research of such exceeding delicacy, has arrived at the conclusion that Arcturus sends us about as much heat as would be received from a three-inch cube full of boiling water, and placed at a distance of three hundred and eighty-three yards. Vega, which shines, according to Sir J. Herschel, with about two-thirds the light of Arcturus, gives out about the same proportionate amount of heat.* But in other instances the heat-giving power of a star has not been found proportional to the amount of light it emits.

The variation of many fixed stars in lustre at once forms a new bond of association between the stars and the sun — which we have seen to be in reality a variable star — and suggests interesting inquiries as to the existence of variation in the emission of heat. Some of the stellar variations of light are so much more marked than those noticed in the case of our own sun that we can scarcely conceive how creatures, resembling any with which we are acquainted, could endure the effects of corresponding important variations of heat; nay, in some instances we seem compelled to withhold our belief in the existence of habitable systems around certain fixed stars. The star Eta Argûs, for example, which sometimes blazes out with a light surpassing that of any of the stars in the northern hemisphere, while at other times it falls to the sixth magnitude, can hardly be regarded as fit to be the centre of a system of worlds. I pass over such variable stars as the one which recently blazed out in the Northern Crown, because in a case of this sort the star may be regarded as really a small orb, and its sudden lustre as due to some exceptional occurrence, leading (as the spectrum of the star seemed to show) to a temporary conflagration. But Eta Argûs and Mira Ceti seem to belong to a different category altogether, since it is probable as respects the former, and certain as respects the latter, that their appearance as stars of the leading magnitudes is not accidental, but part of a systematic series of changes.

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* Although these results cannot yet be regarded as numerically exact, it may be interesting to consider the amount of heat given out by Arcturus in relation to the light sent us by this star, the more so as this star seems (from the nature of its spectrum) to resemble the sun very closely in constitution.

The light sent to us by Arcturus is equal to about three-fourths of that supplied by Alpha Centauri, or about $\frac{1}{21,000,000,000}$th part of the light we receive from the sun. Now, Mr. Stone estimates the direct heating effect of Arcturus at 0°.00,000,127 Fahrenheit, making due allowance for the effect of the object-glass in concentrating and absorbing the heat. It will be seen at once that, according to this estimate, the heating power of Arcturus bears a very much greater proportion to that of the sun than the respective light-giving powers of these luminaries bear to each other. This seems to throw some doubt on the correctness of the estimate, either of the light-giving or of the heat-giving power of the star.

It remains only to be mentioned that, besides light and heat, the stars emit actinic rays. This is proved decisively by the fact that the stars can be made to photograph themselves. It has been found, however, that the actinic power of a star, like its heat-giving power, is not by any means proportional to the star's light. So that in this respect, as in the material constitution of the stars, we find specific varieties even amid those very features which indicate most strikingly the general resemblance which exists between the suns constituting the sidereal system.

To sum up what we have learned so far from the study of the starry heavens — we see that, besides our sun, there are myriads of other suns in the immensity of space; that these suns are large and massive bodies, capable of swaying by their attraction systems of worlds as important as those which circle around the sun; that these suns are formed of elements similar to those which constitute our own sun, so that the worlds which circle round them may be regarded as in all probability similar in constitution to this earth; and that from those suns all the forms of force which we know to be necessary to the existence of organized beings on our earth are abundantly emitted. Is it not reasonable to conclude that these suns have not been made in vain? If thoughtful men have reasoned rightly in supposing that the light and heat poured out by the sun upon the planets which circle around him are not wasted — in the case of all the planets except our small earth — by being shed where no forms of life can profit by those abundant supplies, surely the argument is a million-fold stronger in the case of the fixed stars. Though here we cannot, as in the case of the solar system, actually see the worlds about which we speculate, yet the mind presents them clearly before us, various in size, various in structure, infinitely various in their physical condition and habitudes, but alike in this, that each is peopled by creatures perfectly adapted to the circumstances surrounding them, and that each exhibits in the clearest and most striking manner the wisdom and beneficence of the Almighty.

CHAPTER XI.

OF MINOR STARS, AND OF THE DISTRIBUTION OF STARS IN SPACE.

IT has been so long a received opinion that a general uniformity of magnitude and distribution characterizes the stellar system that it is with some diffidence I venture to express a different view. And here let me not be misunderstood. I am fully sensible that it is only in certain popular treatises of astronomy that a belief in any thing like a real uniformity of structure in the sidereal system is attributed to astronomers of authority. It is not any such imaginary theory that I have now to deal with, however, but with opinions which have found a place in the works of astronomers from whom I very unwillingly differ.

I propose to exhibit the reasons which have led me to believe that, so far from knowing the real figure of the sidereal system, astronomers have not been able to penetrate to its limits in *any* direction; that leading stars, such as those discussed in the preceding chapter, are distributed throughout space to the very farthest limits and beyond the very farthest limits that our most powerful telescopes can attain to; that the stars are arranged in groups and clustering aggregations, in streams and whorls and spirals, in a manner altogether too complex for us to hope to interpret; and that in these aggregations stars of all degrees of real magnitude are mixed up, from suns as large as Sirius down to orbs which may be smaller than any of the primary planets of the solar system.

Now let us consider step by step the evidence we have on these points.

We know, from the existence of double, triple, and multiple stars, in which the components are often very unequal in splendor, that combinations of stars exist in which one or two may be suns like our own, while the rest, or some of the rest, are relatively minute. This, however, has of course long been known; and it is only as a preliminary step in the investigation that I here advance so trite an instance.

Next let us consider such star-clusters as contain orbs of the eighth or ninth magnitude, besides a multitude of minute stars. These clusters must of course be regarded as lying within the sidereal system, since no external galaxies could reasonably be supposed to contain orbs so infinitely transcending even Sirius in magnitude as to shine from beyond the enormous gap separating us from such galaxies with a light exceeding that derived from many stars within the sidereal system. Now, regarding these clusters as forming part and parcel of the sidereal system, we find in the existence of multitudes of minute orbs within their range a proof that diversity of magnitude in schemes of associated stars is to be regarded as a feature of certain parts, at any rate, of our galaxy; and we shall therefore be the less surprised if we should find reason for believing that it is a *characteristic peculiarity* of the galactic system.

Now, with regard to the nebulæ (resolvable and irresolvable), and their claim to be regarded as external galaxies, I shall have much to say farther on; but I may remark, in passing, that we have precisely the same reasons for believing that many of these objects lie within the range of the solar system as have been already considered in the case of star-clusters. Their component stars, to be visible at all, *must* fall within the range of distance which astronomers have assigned to the boundaries of the galaxy, since some stars even within that range cease to be separately visible in the most powerful telescopes man has yet constructed. So that when in these objects we see a few or many distinct stars, and a mass of nebulous light which we judge to proceed from an indefinitely large number of minute stars, we again have very decided evidence of the fact that in one and the same region of the sidereal system there may exist leading stars (so to speak) and innumerable stars relatively minute.

With considerations such as these (and I might add many others) to guide us, let us proceed to consider the teachings of the Milky Way itself, that we may see whether that wonderful zone indeed represents, as has been thought, the sidereal system itself, or only an aggregation of minute orbs altogether insignificant, separately, in comparison with our sun or Sirius, Aldebaran or Betelgeux, Vega or Arcturus.

The star-gauging of Sir W. Herschel, interpreted according to his hypothesis of stellar distribution, pointed to an extension of the Milky Way laterally to a distance exceeding some eighty times that which separates us from the first-magnitude stars. So that, regarding sixth-magnitude stars as on the average about ten times as far from us as those of the first magnitude (the usual estimate), we see that the outermost parts of the galaxy must lie (according to Sir W. Herschel's theory) about eight times as far from us as the sphere of the sixth-magnitude stars.

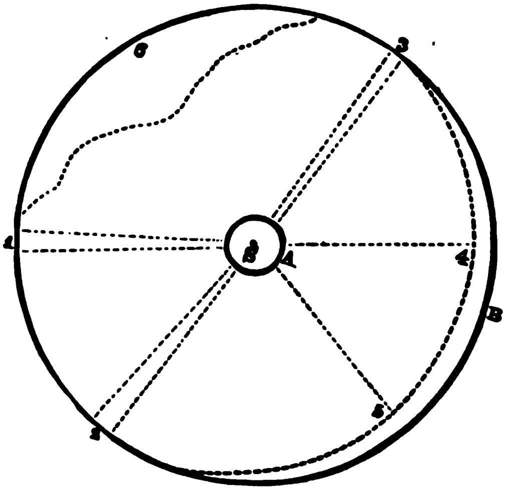

Fig. 2.— The Galactic Cloven Flat Ring (plan).

Now, Sir John Herschel was led by his observations of the southern heavens to so far modify his father's theory as to describe the Milky Way as probably shaped like a flat ring, the stars down to the tenth magnitude being in a sense dissociated from the ring, while he regarded the probable distance of the outermost limits of the ring as seven hundred and fifty times instead of but eighty times the mean distance of the first-magnitude stars. This difference of opinion, it may be remarked, though obviously not surprising when we consider the enormous difficulty of the problem presented by the sidereal system, is yet sufficient to indicate the probability that an important error has been made in the hypothesis which underlies the accepted theories respecting the galaxy. But, be this as it may, in regarding the Milky Way as shaped like a flat ring (cloven through one half of its circumference) whose medial section resembles generally the space between the dark concentric circles in the accompanying figure (in which SB equals eight times SA), I have not adopted a structure which *exaggerates* the difficulties presented by the disk or ring theory of the Milky Way. The cross section would be somewhat as shown in Fig. 3.

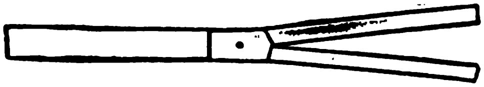

FIG. 3.—The Galactic Cloven Flat Ring (section).

Now, accepting this modified figure, as better according with the results of star-gauging than Sir W. Herschel's theory that the Milky Way forms a cloven disk, let us consider whether any peculiarities of the Milky Way seem to oppose themselves to this interpretation of its structure.

In the first place, then, there is a gap or rift extending right across the single part of the Milky Way in the constellation Argo; so that we must conceive that from S toward 1, in Fig. 2, the flat ring is broken through by some such rift as is indicated by the broken lines in that direction. Next there is, in the constellation Crux, a pear-shaped vacuity of considerable size, and bounded by well-defined edges; so that we must conceive that from S toward 2 (Fig. 2) the flat ring is tunnelled through by some such passage as is indicated by the dotted lines in that direction. A similar tunnelling, but of different cross-section, must exist in direction S 3 (as shown by the dotted lines) to account for the dark gap in the constellation Cygnus. Next, where the Milky Way is double, a large portion of one branch is discontinuous, so that the upper part of the double portion of the ring in Fig. 2 must be supposed removed between the broken lines from S to 4 and 5. Over the so-called double stream there are in places strange convolutions, in others numerous branching and interlacing streams, whose complexity indeed defies description; so that the portion 3 B 2 of the ring must be supposed corrugated in the strangest way, and further to throw out plane and curved sheets of stars presented tangentially toward S. Lastly, the single portion of the Milky Way is very faint indeed toward 6, so that here we must conceive its figure trenched in upon in the way indicated by the dot-and-peck line.

Thus, even without considering a multitude of minuter peculiarities of structure, we are led to the conclusion that the Milky Way, judged according to the fundamental hypothesis of Sir W. Herschel, has some such shape as I have endeavored to exhibit in the accompanying figure. Although I have not indicated here the corrugations of the ring, nor a tithe of the various overlapping layers which would be required to account for the appearance of the Milky Way between Centaurus and Ophiuchus, yet the deduced figure is by no means inviting in its simplicity. It is, however, absolutely certain that the sidereal system, as far as its more densely aggregated star-regions are concerned, has some such figure as this, if we are to accept the principle of Sir W. Herschel's star-gaugings.

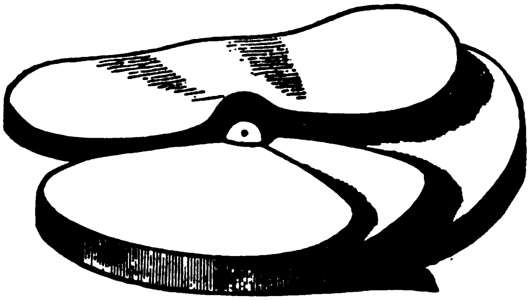

Fig. 4.—The Galactic Flat Ring, modified in accordance with the observed peculiarities of the Milky Way.

Now, in turning our thoughts to the recognition of a more simple explanation of observed appearances, it will be well that we should consider some peculiarities of the Milky Way which we have not yet attended to. In the first place, I would invite attention to a peculiarity observed by Sir John Herschel in different parts of the galaxy — the fact, namely, that in places the edge of the Milky Way is quite sharply defined. One half of a telescopic field of view may be quite clear of stars, or show only a few straggling orbs, while the other half presents what has been called a "Milky Way field"— that is, a region profusely sprinkled with stars, the boundary between the two portions being well defined. When we see that a cluster of objects presents a well-defined edge, what conclusion do we draw as to the position of the object? Is it not in such a case absolutely certain that the distance of the cluster enormously exceeds the distance between its component parts — or, in other words, that the observer is far outside the cluster? Many instances will at once suggest themselves to the reader in illustration of this remark.

We conclude, then, that these portions of the Milky Way, at any rate, whether they be regarded as projections or nodules, are definite clustering aggregations very far removed from us. Other parts of the Milky Way *may* also be removed bodily, so to speak, to enormous distances, because a cluster which has not a definite edge may be as far removed as one which has; but certainly *those* portions are.

Next let us consider what opinion we may found on the existence of dark regions in the Milky Way; and here I refer not merely to such large and obvious vacuities as the coal-sack in Crux or the oval opening in Cygnus, but also to small openings, in which, though they occur even in rich regions of the Milky Way, there is not according to Sir W. Herschel's description, even a telescopic star to be seen.

Judged apart from preconceived opinions, such openings as these, according to all laws of probability, indicate that the portion of the Milky Way in which they occur has not a very great lateral extension. To return for a moment to Fig. 2, it will be seen at once that an aperture extending laterally through a star-system so shaped must have a particular direction and be perfectly straight in order to be visible to observers placed, as we are supposed to be, in the central opening. It is altogether improbable that one such opening should exist by accident, and absolutely impossible that many should.* We are forced therefore to infer that, instead of the enormous lateral extension assigned to the Milky Way, the galaxy has in these places certainly, and elsewhere probably, a lateral extension not greatly exceeding its depth.

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* Sir John Herschel has distinctly indicated this inference, as he has many other matters which make strongly against the received theory of the sidereal system. Nor is he unconscious of their bearing. Apparently unwilling at present to press them to their full extent, he is commonly satisfied by noting that they do not seem to accord with views he has elsewhere dwelt upon.

It is further to be noted that the lucid stars over that zone of the heavens which is occupied by the galaxy show a very decided preference for the parts of that zone which are actually traversed by the Milky Way. For instance, we find no stars above the fifth magnitude, and very few of these, in the Coalsacks, or in the rift which crosses the Milky Way in Argo, or, again, in the space which lies between the two branches where the Milky Way is double. If this is an accident, it is a very extraordinary one, especially when it is remembered that the region where it occurs is the very part of the heavens where stars of all magnitudes may be expected to be most profusely distributed; that the spaces thus left vacant form no inconsiderable aliquot part of that zone; and that, according to the accepted theory, there is no reason for expecting any peculiarity of the sort.

Thus, again, setting aside preconceived opinions, and judging only according to the evidence, we seem led to regard the coincidence as not accidental, but as indicating that there really is a very close association between the bright stars and those small stars forming the milky light, which, according to the accepted theory, would lie so many times farther from us.[*]

Now, if we have not been mistaken so far, it is very clear what views we are to form. If the Milky Way is to be, *first*, a clustering aggregation separated from us by an interval comparatively clear of small stars; *secondly*, so shaped that the cross-section of the stream is everywhere not far from a roughly circular figure; and, *thirdly*, associated very closely with the bright stars seen in the same field of view, then must its structure be somewhat as shown in Fig. 5, in which the disks represent lucid stars (very much exaggerated of course in size), while the fine dotting represents the spiral of relatively minute stars, clustering along the spiral group of leading stars. It will be seen at once how, to an observer placed at S, the various features of the Milky Way can be accounted for by this figure. Toward *a* would lie the gap in Argo; toward *b* two branches, one faint, and in part evanescent through enormity of distance, the other forming the brightest part of the spiral; toward *d* the projection in Cepheus; toward *e* the faint part of the Milky Way in Gemini and Monoceros. The Coal-sacks would be simply accounted for by conceiving that branches seen toward the same general direction, but at different distances, do not lie in the same general plane, and so may appear to interlace upon the heavens. We are not only justified in supposing this, but forced to do so by the way in which the stream of milky light is observed to *meander* on its course athwart the heavens. The branching extensions serve very well to account for the appearance of the Milky Way between Centaurus and Ophiuchus, where the interlacing branches and the strange convolutions and clustering aggregations described by Sir John Herschel are chiefly gathered.

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[*] I may add that, in drawing the maps for my new star-atlas, I have been very much surprised to find how in many cases the position, nay the very shape, of the Milky Way is indicated by the lucid stars which fall on its zone. Although my own views had led me to look for a peculiarity of the sort, it has been much more striking in its character than I had expected.

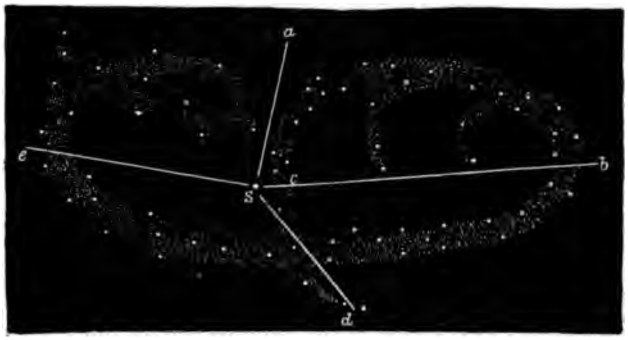


Fig. 5.—The Milky Way regarded as a Spiral.

I would not have it understood, however, that I at all insist on the general shape of the spiral shown in Fig. 5. On the contrary, that curve is only one out of several which might fairly account for the observed appearance of the Milky Way; and I have often felt inclined to doubt whether a single spiral of this sort be in reality the best way of accounting for the observed appearance of the galactic zone. What I do insist upon as most obviously forced upon us by the evidence is, that (1) the apparent streams formed by the Milky Way upon the heavens indicate the existence of real streams in space; and (2) that the lucid stars seen on the stream are really associated with the telescopic stars which form, so to speak, the body of the stream. Whether that stream form a single spiral or several, or *whether, instead of spirals, there may not be a number of closed rings of small stars, placed at different distances from us, and lying in all directions round the medial plane of the galaxy, but more or less tilted to that plane* (*the sun not lying within any one of the rings*), are questions which can only be resolved by the systematic scrutiny of this wonderful zone.

The chief points to be noticed among the considerations flowing from these general views are these:

In the first place, the only marked difference between the stars of the leading magnitudes (say the first ten) lying in the galactic zone, and those lying without it, consists in the fact that the former are associated with countless multitudes of smaller stars, while the latter appear not to have such attendants, or not so many of them. We shall see presently that the extra-galactic stars *are* associated, and in a very intimate manner, with groups of very minute stars — of stars so minute indeed as not to be separately discernible — so that astronomers have been led to regard such groups as external galaxies. But, except in one region, we do not find outside the galactic zone any appearances reminding us of the aspect of the Milky Way itself. In that region lie the two Magellanic Clouds, resembling the Milky Way in their general appearance, but seen when placed under telescopic scrutiny to differ from it in this, that among the minute stars which cause the milky light are numbers of nebulæ, of classes not found commonly, if at all, in the galactic zone.

In the second place, we must conclude that uncounted millions of stars exist which are very minute indeed in comparison with those which we have been led to regard as suns. That these relatively minute orbs may be absolutely large — far larger, for instance, than our own earth — may indeed be accepted as certain. But it is difficult to believe that they subserve purposes similar to those of our own sun. One cannot but see that orbs such as these would not have that permanence of character, as sources of heat-supply, which would seem to be necessary in the case of a real sun. We know, indeed, that among the small stars of the Milky Way there is a proneness to irregular variation which is not recognized, or is altogether exceptional, among the lucid stars. In the neighborhood of the Milky Way, with scarcely an exception, those temporary stars have blazed out which have formed a subject of such perplexity to the thoughtful astronomer. Under what conditions the small orbs in the Milky Way actually exist, whether clusters of them will eventually segregate from their neighbors to form suns, or whether, after long voyaging in spiral and contorted paths under the varying influences of the attractions of leading stars, these minute orbs will, for the most part, be forced to settle down as attendants round the major ones, it is as yet altogether impossible to judge. It may be that they bear the same sort of relation to the leading stars that certain cometic and meteoric families, referred to in Chapter IX., bear to the major planets of the solar system, not being in any case absolutely dependent on any large star, but yet returning in cycles which must be measured by millions of eons, to temporary dependence on one sun after another, until in the course of time, under the action of processes somewhat resembling those I have conceived to take place in the formation of the solar system, the conditions under which they move will have become so far altered as to lead to the breaking up of the Milky Way into distinct systems. Indeed, as Sir William Herschel was led by other considerations long since to point out, there are signs in parts of the Milky Way which would seem to indicate that several such systems have already reached an advanced stage of development.

But perhaps the most important conclusion deducible from the circumstances I have dwelt upon (assuming my interpretation of them to be in the main correct) is this, that we can no longer suppose we have in any direction pierced to the limits of the sidereal system. So long as a general approach to uniformity of distribution was understood to prevail within that system, there was a ready means of determining when the telescopist had reached in any given direction the limits of the system. To use the words of Prof. Nichol, "When an eye is directed toward a prolonged bed of stars, there is no reason to fancy that it has reached the termination of that stratum so long as there appears, behind the luminaries which are individually seen, any milky or nebulous light; such light probably arising always from the blended rays of remoter masses. But if, after struggling long with a nebulous ground, we obtain a telescope that gives us additional light with *a perfectly black sky*, we then have every reason the circumstances can furnish on behalf of the supposition that at length we have pierced through the stratum, a probability, indeed, which can be converted into certainty in only one way — viz., when no increase of orbs follows on the application of a still larger instrument." Sir John Herschel has expressed a similar view, and there can, indeed, be no doubt that, adopting the fundamental hypothesis on which accepted views are founded, the test above described is an absolutely certain one.

But, if, instead of penetrating farther and farther into space when "struggling long with a nebulous ground" (to use Prof. Nichol's striking but somewhat incorrect expression), we have in reality only been searching with more and more minuteness within a definite cluster or stream of stars, we can no longer come to the conclusion he has insisted upon. We have reached the limits of minuteness which the stars of the cluster or stream attain to; we have learned perhaps all that we can learn about that cluster or stream; but we can no more be said to have reached the limits of the sidereal system in that direction than we can be said to have reached the outermost bounds of the universe in the direction of the cluster in Hercules, when that magnificent object has been thoroughly resolved with the telescope.

Here, then, if I have seemed to narrow the limits of the sidereal scheme by bringing the star-myriads of the Milky Way, which had been regarded as many times farther from us than the lucid stars, into direct association with these luminaries, I make amends by pointing out that in all probability the limits of the sidereal system lie far beyond the range of the most powerful telescopes man has yet constructed. In fact, there is here a somewhat singular interchange of position between the new and the accepted theories. According to the views usually accepted, the small stars in the Milky Way are really as large, on the average, as the lucid stars, whereas, according to my views, they are relatively minute. But, according to the accepted theories, the scattered stars of very low magnitudes in the extra-galactic heavens must be regarded as relatively minute, since it has been rendered certain, according to those theories, that the limits of the sidereal system are relatively close in this direction, and we cannot suppose these stars to lie beyond those limits (as they must do, if really large). Now, according to my views, there is nothing to prevent these minute stars from including among their number orbs as vast as Sirius, or many times vaster. Nay, even within the galactic zone itself there are stars to which my theory gives as noble proportions as the accepted views. For, in the southern Coal-sack, there are minute telescopic stars, as Sir John Herschel tells us, and these orbs, according to the accepted views, must be regarded as belonging to the galactic circle, though inexplicably segregated from their fellows. According to the views I have been led to form, many of these telescopic stars must be regarded as suns lying far beyond the galactic spiral, or perhaps associated with outer whorls of this spiral which no telescope made by man can ever reveal to us.

And this leads me to consider two phenomena which are altogether inexplicable, I conceive, on any theory except mine.

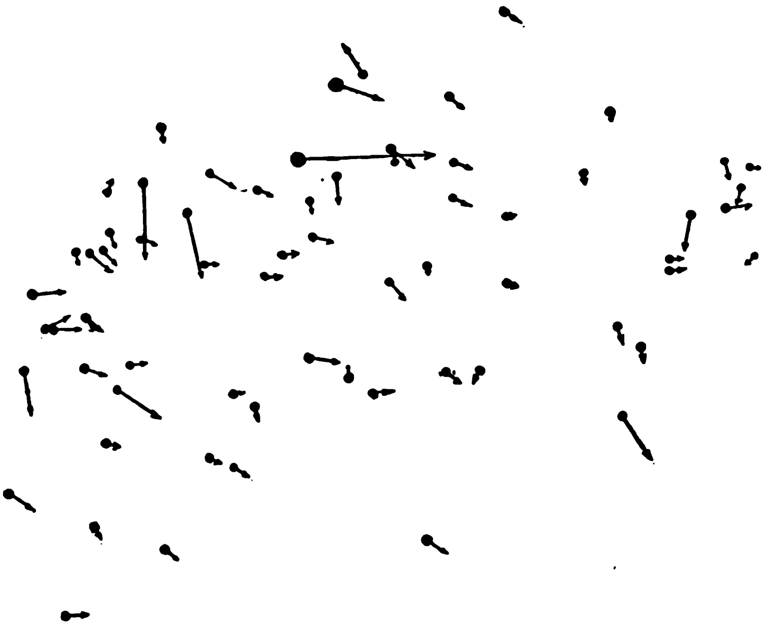
The first is the existence of excessively faint streams of light — star-streams doubtless, though the components are not separately visible — in certain regions of the heavens. Sir John Herschel, who detected this strange phenomenon, speaks of the streams as so very faint that the idea of illusion has continually arisen subsequently; yet he dwells far too clearly on the characteristics of the phenomenon for any doubt to remain as to its reality. The faintest possible stippling of the field of view — the minute points of light being obviously *there,* though it was impossible to see them individually — a mottling which moved with the stars as he moved the tube to and fro, such are the terms in which Sir John Herschel speaks of this interesting phenomenon.

Now, no doubt whatever can exist that, if these faint streams really belong to the sidereal system, they are left altogether unaccounted for by the ordinary views respecting the structure of that system. There is no continuity between the stars composing them and even the minutest telescopic stars visible in the same general direction; so that a vast void must separate them from the outermost of those telescopic stars. According to my theory, they simply belong to outlying whorls of the spiral galaxy, and the telescopic stars seen upon them bear the same relation to them that the lucid stars bear to the Milky Way.

The second point is perhaps even more striking. In certain directions Sir John Herschel recognized the existence of two or more distinctly-marked classes of stars, as though, he says, definite sets of stars, separated by comparatively void intervals, lay in those directions. It is clear that this association of the stars into sets is as distinctly opposed to the views ordinarily accepted as it is obviously an arrangement to be expected according to my theory of the constitution of the sidereal system.

Quite early in my consideration of the subject I am now upon, the idea suggested itself to me that in the proper motions of the stars we have a means of forming an estimate of the distances of these orbs; and, further, of detecting any laws associating them together, whether into streams or clusters; and that the evidence thus obtained was likely to be in many respects more trustworthy than that afforded by the apparent magnitudes of the stars. Two processes of inquiry suggested themselves. The first consisted in a careful comparison of the mean motions of stars of different apparent size, in order to determine whether, on the average, small stars are so far off that we can look upon them as in reality no smaller on the average than those which appear larger. The second consisted in charting down the proper motions, so as to detect any signs of star-drift which might haply appear in different parts of the heavens. I confess that I had not by any means expected results so strikingly confirmatory of my views as those I actually obtained.

The first method of inquiry, instead of giving an average amount of proper motion to the smaller stars somewhat, or perhaps even considerably, greater than was to be expected, according to the theory which sets these stars at an enormous distance, actually gave them a mean motion *equal* to that of stars of the first three magnitudes. It became evident, then, that not only are small stars (I am here speaking of stars visible to the naked eye) mixed up as I had thought with bright stars visible in the same general direction, but that distance is less available to explain the smallness of the stars even than I had supposed. I had thought that certainly a large proportion of the small stars must in reality be very far from us; but it appeared that the proportion of stars whose smallness is so to be accounted for is in reality exceedingly minute. There must therefore be myriads of really small stars for every leading orb.



Drift of the Stars in the Constellations Cancer and Gemini.

The second method of research led to the strange result that in many parts of the heavens a community of motion can be recognized, among star-groups far larger in extent than I had expected to find thus drifting through space. Knowing that, whatever view we form of the sidereal universe, we must yet recognize the fact that in every direction stars at very different distances must be visible, I had not hoped to find over any large region of space the traces of a community of motion. Nor even in small regions had I hoped to recognize very decided traces of star-drift, because I was conscious that, even with three or four stars really forming a drifting group, there would nearly always be found three or four others, either much farther off or much nearer, and altogether dissociated from the drifting set. Indeed, I imagined, when I began the inquiry, that the most remarkable instance of star-drift in the heavens was that detected (though differently explained) by Baron Mädler in the constellation Taurus.

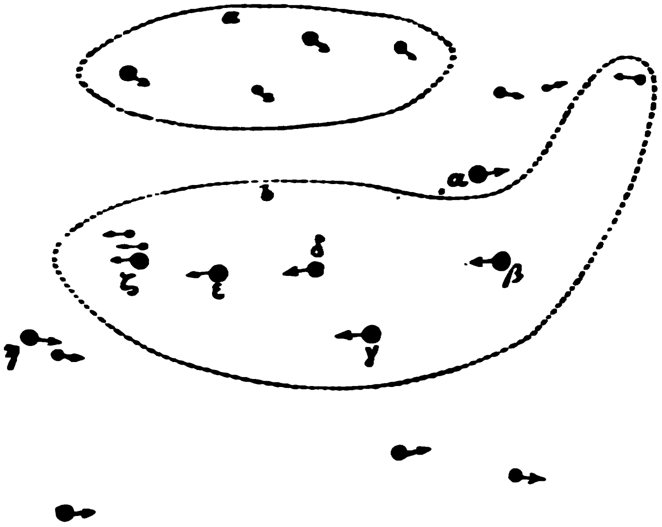


FIG. 6.—Observed Proper Motions of Stars in Ursa Major and Neighborhood.

I found, however, that in other regions a far more obvious tendency to drift can be recognized. Perhaps the most remarkable instance of all is that illustrated in the accompanying plate. This picture represents the motions in the constellations Cancer and Gemini. It will be noticed that though here and there stars apparently not belonging to the system appear in the same range of view, yet the star-drift is unmistakable. The general parallelism of motion is very striking; and the difference in the amount of motion observed in different stars is only what was to be expected in a star-group whose range in distance, if equivalent to its lateral extent, must be such as fully to account for the range in the amount of apparent motion.

Fig. 6 exhibits one out of many parts of the heavens in which different sets of stars are observed to be drifting in different ways.

It will be seen that here there are three sets — those included in the space *a*, those in space *b*, and those left unenclosed, which are very obviously drifting, each in its special direction. The stars within the space *b* are β, γ, δ, ε, and ζ, of the Greater Bear, with three smaller stars. Their drift is, I think, most significant. If in truth the parallelism and equality of motion are to be regarded as accidental, the coincidence is one of a most remarkable character. But such an interpretation can hardly be looked upon as admissible, when we remember that the peculiarity is only one of a series of instances, some of which are scarcely less striking. One of these is presented in the accompanying figure in which the proper motions in the stars α, β, and γ, Arietis, and four other stars in the neighborhood, are exhibited.*

Here β and γ may be regarded as drifting with α, but having a motion of their own in addition, sufficing to account for the want of strict parallelism between their apparent motion and that of α. The other stars seem obviously to belong to the same system.

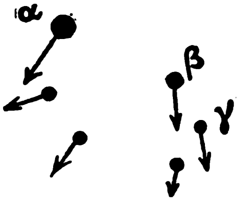


FIG. 7.—Observed Proper Motions of Stars in Head of Aries.

I am led, by the facts which have here been briefly considered, rather to urge those who have time and inclination to inquire carefully into the minuter details of the sidereal heavens than to insist on any views of my own. While I recognize the wisdom and necessity of that course which the Herschels adopted in taking a wide view of the sidereal system, and in dealing rather with general results than with special peculiarities, I think the time has come when another course is possible and advisable. The Herschels having surveyed the field of heaven, it behooves us now to go over it with a close and searching scrutiny. To consider averages *now* is to level the scarcely perceptible undulations in our field of research, as well as its better-marked ridges or depressions; whereas we require, on the contrary, to exaggerate the variations of level, so that we may determine with more certainty what are the peculiarities presented by that most interesting field to man's contemplation. Or, to change the illustration, and to quote the words of the greatest living master of that kind of research which I have been advocating, "We must not be deterred from dwelling consecutively and closely on these speculative views by any idea of their hopelessness which the objectors against 'paper astronomy' may entertain, or by the real slenderness of the material threads out of which any connected theory of the universe has (at present) to be woven. '*Hypotheses fingo*' in this stage of our knowledge is quite as good a motto as Newton's '*Non fingo*'—provided always they be not hypotheses as to modes of physical action for which experience gives no warrant."*

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* In all these figures the proper motion indicated by the length of the arrow attached to a star corresponds to the star's motion in thirty-six thousand years.

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* From a letter addressed by Sir J. Herschel to the present writer August 1, 1869.

CHAPTER XII.

THE NEBULÆ: ARE THEY EXTERNAL GALAXIES?

IN THE last chapter I have indicated reasons for believing that the sidereal system extends far beyond the range of the most powerful telescopes man has yet been able to construct. It need hardly be said that, supposing this view to be correct, we cannot possibly see any external galaxies, unless they surpass our own many thousands of times in richness and splendor. Every analogy that we have for our guidance points to the conclusion that, if our galaxy have limits, and there exist in space other galaxies, then those outer systems must be separated from ours by spaces exceeding the dimensions of the several galaxies many thousand or many million fold in extent. We know that the distances separating the satellites from their primaries exceed in an enormous ratio the dimensions of the satellites. The distances separating the planets from each other exceed in an enormous ratio the dimensions of the planets. The distances separating our solar system from others enormously exceed the dimensions of the various solar systems. And we may conclude that in all probability the distances separating our sidereal system from other similar systems in space must exceed in an enormous ratio the dimensions of our galaxy, and of all other such systems.

That the sidereal system has limits I do not doubt. Of course it *may* be coextensive with space that is absolutely infinite in extent; but we have no reason for believing that, in rising step by step, from system to system, until we have reached the highest class of system known to us, we have reached the real summit of that perhaps altogether limitless range of steps. We know, indeed, that if light do not suffer extinction in traversing space (and we have as yet no evidence that it does), the extent of the sidereal system *must* be limited, since otherwise the whole of the starlit sky should shine with the brilliancy of sunlight.* And we may carry this argument even further. For, though the sidereal system should be limited, but other systems similar to it spread throughout the infinity of space, there would still result this ineffable blaze of light, surpassing the light of day as greatly as the vault of heaven surpasses the disk of the sun. And this again would be true, though this system of systems were limited in extent, but surrounded by similar systems of systems in the infinity of space. And so on, let the order of systems which finally becomes infinite in number be what it may. There is only one way to escape from this limitless series of system-orders — that is, by accepting as true the hypothesis that light suffers extinction as it voyages through space. But it is worth noticing, when we are actually dealing with the infinity of space, and when, therefore, limitless conceptions are not paradoxical, but in reality as available for our purposes as finite conceptions would be, that if we do adopt the belief in an infinite succession of orders of systems, that is, first satellite-systems, then planetary systems, then star-systems, then systems of star-systems, then systems of systems of star-systems, and so on to infinity, and if we accept as true of this infinite series what we know to be true of the part within our ken, viz., that the distance between the components forming any system is indefinitely great compared with the dimensions of those components, we no longer have as a conclusion that the whole heavens should be lighted up with stellar (that is with solar) splendor; even though, in this view of the subject, there are in reality an infinite number of stars, just as in the view according to which the sidereal system extends without interruption to infinity.*

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* This is, perhaps, obvious; but, if not, the following proof may be accepted: Let the whole of space be conceived divided into spherical shells, having our earth at their centre, the thickness of each shell being τ. Then taking two shells, one at a distance r, the other at a distance r' (both r and r' much greater than τ), we see that the number of stars in these shells will be proportional to $r^2 \tau$ and $r'^2 \tau$ respectively; that is, will be independent of the thickness of the shell and vary as the square of its radius. (Here I am not concerned with those departures from uniformity which I have considered in the last chapter, because I suppose each shell large enough to include within it all varieties of distribution and aggregation. This applies, also, to what follows.) Now, the average apparent size of the stars of one shell will be to the average apparent size of stars in the other in the inverse proportion of the respective radii of the shells, the intrinsic brightness of the light received from the stars of each set being equal. Thus the total amount of light from the stars of one shell is to the total amount of light from stars in the other, as $r^2 \tau \times \frac{1}{r^2} : r'^2 \tau \times \frac{1}{r'^2} = 1 : 1$. Hence, supposing the amount of light received from one shell to be $\frac{1}{k}$th part of that which would be received if the whole celestial sphere were as bright as the sun's, that is as a star's disk—k being inconceivably large, the amount received from the other is also $\frac{1}{k}$th of this amount, and the total from all the shells must, therefore, be $\frac{1}{k}+\frac{1}{k}+\frac{1}{k}+\frac{1}{k}$ ——— to infinity.

Now, by taking k terms of this series (or k shells out of our infinite series of shells), we should get unity, that is, the whole heavens lighted up with starlight or sunlight. There would be a proportion of stars in the same visual line and so hiding each other; but, since we can take 2 k, 3 k, or *infinity* times k if need be, there can be no doubt the whole heavens would be lighted up with solar brightness.

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* It is clear that we no longer get, as in the previous note, a series of equal small terms. If we take our infinite series of shells as before, we get for the sidereal system n times $\frac{1}{k}$ where n is finite and therefore $\frac{n}{k}$ finite. We must indeed assume $\frac{n}{k}$ to be small, and so of other similar ratios presently to be dealt with. With respect to the system of systems we have these considerations to guide us,— any of the spherical shells within this system must supply to our skies an amount of light indefinitely less than one of the shells within the sidereal system itself, say $\frac{1}{k'}$th part only, k' indefinitely large; but the number of shells falling within that system is very much greater, say n' times as great where n' is finite. Therefore we get for the total amount of light coming from the system of systems a quantity proportional to $\frac{nn'}{kk'}$, and so for the system of system of systems we get a quantity proportional to $\frac{nn'n''}{kk'k''}$, where k'' is indefinitely large, n'' very large. And for each successive order we get a multiplier of the form $\frac{N}{K}$, where K is indefinitely large and N very large indeed. Suppose $\frac{\nu}{x}$ to be the largest of all these multipliers, then the total amount of light received from the infinite system of systems is proportional to less than

$$\frac{n}{k}\left(1+\frac{\nu}{x}+\frac{\nu^2}{x^2}+\frac{\nu^3}{x^3} \text{ —— to infinity}\right).$$

(in which ν is supposed to be less than x), i. e., to less than $\frac{n}{k}\left(\frac{x}{x-\nu}\right)$, a finite quantity, which will even be minute if k and x are severally much greater than n and ν.

This particular mode of escaping from the difficulty suggested by the illumination of the heavens, without adopting the theory that light suffers extinction in its passage through space, occurred to me while I was preparing a series of papers entitled "A New Theory of the Universe," which appeared in *The Student* in the spring of 1869, and I there exhibit the considerations just dealt with. I was much pleased

But whether we adopt this or any other view of the way in which external systems are arranged, this at any rate is certain, that if the stars at the outer parts of our own sidereal system be beyond the ken of our most powerful instruments — and I have shown that there are strong reasons for this conclusion — then the component suns of external galaxies cannot by any possibility be visible. So that, according to this view, all resolvable nebulæ, at least, must be dismissed from the category of external galaxies. Nor will it be thought probable that irresolvable nebulæ are external galaxies, if once that view of the extent of the sidereal system is adopted.

But there are independent considerations, on which I prefer now to dwell, for believing that all the nebulæ belong to the sidereal system.

It will hardly be necessary, let me remark in passing, for me to point out how this matter is associated with the subject of other worlds. It is true that, when once it is admitted that there are external galaxies, it may be looked on as a matter of small importance (so far as the subject of this treatise is concerned) whether we can actually see those galaxies or not. I am not, for instance, in the same position as Dr. Whewell, who assigned to the nebulæ what I take to be their true place in the universe, with the express object of overthrowing the belief that

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to find, from a letter of Sir John Herschel's, that the same idea had suggested itself to him; as I was thus encouraged to believe that I had not gone very far astray in the whole series of papers, whereof the matter in question had seemed to me the most speculative portion. The following are the words in which Sir John Herschel, writing in ignorance of my having adopted the same view, expresses the ideas above dealt with: "One of the arguments advanced in favor of the spatial extinction of light was that, if there is not such extinction, the whole heavens ought to be one blaze of solar light — admitting the universe to be infinite, because it was contended that there could then be no direction in space in which the visual ray would not encounter a star (i. e., a sun). This argument is fallacious, for it is easy to imagine a constitution of a universe literally infinite which would allow of any amount of such directions of penetration as *not* to encounter a star. Granting that it consists of systems subdivided according to the law that every higher order of bodies in it should be immensely more distant from the centre than those of the next inferior order — this would happen. Thus, in our own, the moon is very near the earth, the satellites to their primaries. These primaries are immensely more distant from the sun, *their* centre; the fixed stars again still *more* immensely more remote from the sun. Suppose *our* system to terminate with the visible fixed stars; then imagine a system of such systems as remote from each other, *in comparison with their own dimensions*, as the distance of the fixed stars in comparison with the planetary system; such systems seen from each other would subtend no greater angle than a star seen from the sun — and so on."

there exist other galaxies as vast as the sidereal or vaster, thronged with suns which are severally the centres of planetary systems, within which again are worlds as well suited to be the abode of life as this earth on which we dwell. But, though my purpose is different from his, it is equally necessary that I should insist on the true position of the nebulæ. Because, if these objects form indeed part of the sidereal system, the relations they present are of extreme importance. They exhibit to us within the bounds of our galaxy systems altogether different from the solar system, and thus suggest ideas of other classes of worlds peopled with their own peculiar forms of life, as distinct, perchance, even in their general characteristics, from any found amid the systems circling round stars, as the forms of life in Venus or in Mars must be in their special characteristics from those existing on our own earth.

Freed from those analogies which led the elder Herschel to regard the stellar nebulæ — resolvable and irresolvable* — as external star-systems, let us consider the relations presented by these and other nebulæ, without reference to preconceived opinions.

We must first pay attention to one of the most striking of the discoveries which the spectroscope has yet enabled man to make — the discovery that certain nebulæ are gaseous. It is necessary to consider this significant discovery, rather than those which were the first to exhibit the real place of the nebulæ in our scheme, because we shall thus be able to divide the nebulæ at once into two great classes, instead of being led to this arrangement by following out the history of those long processes of research by which the two great orders of nebulæ were long since separated from each other under the piercing scrutiny of Sir William Herschel.

The reader will see how the spectroscope could at once resolve a question which ordinary observations would be all but powerless to deal with. The nebulæ being self-luminous, the nature of the matter which is the source of their light would be shown by the character of the spectrum, as distinctly as though that matter were actually present in the laboratory of the spectroscopist.

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* By irresolvable stellar nebulæ, I mean those nebulæ which, though not resolvable into stars, yet present the characteristic features which lead astronomers to believe that only increase of telescopic power is needed in order to effect resolution.

Mr. Huggins thus describes the observation which first revealed the true nature of certain orders of the nebulæ. The object under examination was a nebula in Draco, belonging to the class of planetary nebulæ: "On August 19, 1864, I directed the telescope, armed with the spectrum apparatus, to this nebula. At first I suspected some derangement of the instrument had taken place, for no spectrum was seen, but only a short line of light perpendicular to the direction of dispersion (that is, to what would in the case of solar light be the *length* of the spectrum). I then found that the light of this nebula, unlike any other ex-terrestrial light which had yet been subjected by me to prismatic analysis, was not composed of light of different refrangibilities, and therefore could not form a spectrum. A great part of the light from this nebula is monochromatic, and after passing through the prisms remains concentrated in a bright line, occupying the position of that part of the spectrum to which its light corresponds in refrangibility. A more careful examination, however, showed that — a little more refrangible than the bright line, and separated from it by a dark interval — a narrower and much fainter line occurs. Beyond this again, at about three times the distance of the second line, a third exceedingly faint line was seen. The positions of these lines in the spectrum were determined by a simultaneous comparison of them in the instrument, with the spectrum of the induction-spark taken between electrodes of magnesium. The strongest line coincides in position with the brightest of the air-lines. This line is due to nitrogen. . . . The faintest of the lines of the nebula agrees in position with a line of hydrogen. The other bright line was not found to correspond with a known line of any terrestrial element. Besides the bright lines, an exceedingly faint spectrum was just perceived for a short distance on both sides of the group of bright lines. Mr. Huggins suspected that this was not uniform, but crossed with dark spaces. Subsequent observations on other nebulæ* induced him "to

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* One of the most interesting of Mr. Huggins's researches into the subject of the light of nebulæ is his attempt to determine its intrinsic brilliancy. By comparing the light of certain gaseous nebulæ with that of a sperm-candle (of the size called six to the pound), he found that these objects, assumed to be continuous, shine with a light varying in

regard this faint spectrum as due to the solid or liquid matter of the nucleus, and as quite distinct from the bright lines into which nearly the whole of the light from the nebula is concentrated."

Thus was solved a problem which had, for the best part of a century, perplexed astronomers. There was not, indeed, a full answer to all the questions of interest associated with the problem. But it had been laid down by Sir William Herschel, as a legitimate conclusion from observation, that certain orders of the nebulæ are gaseous, and astronomers had ranged themselves for and against this proposition. Telescopic improvements had seemed at length to turn the scale in favor of those who held Sir William Herschel to have been mistaken. Already the problem had seemed all but definitely settled. And then in a moment this observation by Mr. Huggins had reversed the whole matter. It was now established, beyond all possibility of future question, that, on the main point, the greatest of modern astronomers had been altogether in the right.

The orders of nebulæ which give a spectrum of bright lines, would seem from Mr. Huggins's observations to be (i.) the planetary nebulæ, (ii.) the ring nebulæ, (iii.) the irregular nebulæ. The spiral nebulæ seem, for the most part, to give a continuous spectrum, but some of these objects give the bright-line spectrum indicative of gaseity. The orders of nebulæ which give a continuous spectrum appear to be the following: (i.) star groups, (ii.) clusters, regular and irregular, and (iii.) easily resolvable nebulæ. Of the irresolvable nebulæ a large proportion seem to be gaseous.*

Here, then, we find the nebulæ ranged into two important divisions, apparently separated by a distinct line of demarcation. Yet one is tempted to inquire whether these divisions may not in reality run into each other, by the fact that among nebulæ

intrinsic brilliancy from the 1,500th to the 20,000th of that of such a candle. By a strange misconception, Mr. Lockyer, in discussing Mr. Huggins's result, speaks of the comparison as though it related to the absolute brightness of the nebulæ, saying that "such a candle a quarter of a mile off is 20,000 times more brilliant than the nebula." Mr. Huggins's result is wholly distinct from this, and much more important. His comparison relates to the intrinsic luminosity of the nebular substance, not to the *quantity* of light received from the nebulæ. (The distance of the candle in Mr. Huggins's observations is not considered in the result; it was a mere matter of convenience.)

* The following classification of nebulæ in this respect, by Lord Oxmantown, is interesting as indicating the results of observations made

of certain orders are objects belonging to both divisions. And the fact that, beneath the bright-line spectrum of the gaseous nebulæ, a faint continuous spectrum may be seen, seems also to point in the same direction. We know that, so far as the telescopic appearance of the nebulæ is concerned, there is very striking evidence of a gradual progression from clusters to irresolvable nebulæ, and, therefore, we are led to inquire, whether the spectroscope conveys a similar lesson.

Now, this question could only be answered satisfactorily by the observation of a series of nebulæ having spectra progressively varying, from bright lines on an almost invisible continuous spectrum to a continuous spectrum with the same bright lines superposed on it, but almost imperceptible, because their brightness so little exceeded that of the continuous spectrum. We have not evidence of such completeness. But Lieutenant Herschel has observed in the southern heavens a clustering nebula with a continuous spectrum, on which he could just detect the three bright lines seen in the spectra of the gaseous nebulæ. And, so far as this evidence extends, the conclusion is obvious, that the various orders of nebulæ are orders of but a single family. It will be seen presently that this conclusion, which is strikingly corroborated by other evidence, has a very important bearing on the views we are to form respecting the relations between the nebulæ and the sidereal system.

The first process by which we must attempt to form a correct estimate of the nebular system corresponds to Sir William Herschel's process of star-gauging. We must inquire according to what general laws the nebulæ are spread over the vault of heaven.

Now, when this is done, it appears that there is a well-marked

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with so powerful an instrument as the great Parsonstown telescope (the six-feet reflector):

	Continuous Spectrum.	Gaseous Spectrum,
Clusters	10	0
Certainly or probably resolved?......	5	0
Certainly or probably resolvable?.....	10	6
Blue, or green, no resolvability........	0	4
No resolvability detected.............	6	5
Total observed	31	15

Adding nebulæ not observed at Parsonstown, there are in all 41 which exhibited a continuous spectrum, and 19 which gave a spectrum indicative of gaseity.

peculiarity in the arrangement of the nebulæ, a peculiarity as striking as the existence of the galactic circle itself. *The nebulæ seem to withdraw themselves from the neighborhood of the galaxy.* In the northern heavens they cluster very definitely toward the pole of the galaxy; in the southern they are arranged in streams and clustering aggregations, but the galaxy itself is, in either case, left almost clear of nebulæ.

If this peculiarity is accidental, the coincidence involved is most remarkable. Had there been a zone of nebulæ, and that zone had shown a tendency to coincidence with the Milky Way, the relation would have been held strikingly indicative of a real association between the nebular and the sidereal systems. But is the direct converse of this relation more likely to be the effect of chance? Have not observers and experimenters concluded (in every other similar instance) that a law of contrast is as indicative of a real connection as a law of association? It is surprising, therefore, that nearly all astronomers, who have considered the relation in question, have regarded it as affording strong evidence that the nebular system is wholly dissociated from the sidereal.

Next let us turn to special features. In the first place, let us inquire whether the different orders of nebulæ exhibit any peculiarities of arrangement.

We find that clusters exhibit a very marked preference for the neighborhood of the Milky Way; resolvable nebulæ seem to prefer the galactic zone, but not in so decided a manner; and it is only among the irresolvable nebulæ that we recognize that withdrawal from the Milky Way which had seemed characteristic of the whole nebular system, before we considered its several orders. The fact that the irresolvable nebulæ form about four-fifths of the total number will account for the circumstance that a peculiarity really appertaining to that order alone should appear to belong to the whole system of nebulæ.

Again, the planetary and irregular nebulæ are found to affect the neighborhood of the Milky Way. I have already mentioned that these objects are gaseous.

It is easy to see what general conclusions may be deduced from the peculiarities here touched upon. Obviously the first shows us most distinctly that there is a relation between propinquity to the Milky Way and the character of nebulæ as respects resolvability — a relation which points in the most decisive manner to the existence of a close association between the sidereal system, of which the Milky Way certainly forms part, and the nebular system, from which clusters and resolvable nebulæ cannot reasonably be separated. It is equally obvious that the second peculiarity indicates the existence of a close association between the Milky Way and the character of the nebulæ as respects gaseity; a relation which brings all the gaseous nebulæ into close association with the sidereal system, since we know that among the extra-galactic nebulæ there are many which are principally formed of the very same gases which appear in the irregular and planetary nebulæ. When we consider that those peculiarities of configuration and of constitution which have alike seemed to indicate that the various orders of nebulæ merge into each other by indefinable gradations are both associated, in a very distinct manner, with the most marked peculiarity of the sidereal system, and when to this we add what has been already suggested by the relation of contrast between the irresolvable nebulæ and the Milky Way, the conclusion seems forcibly impressed upon us that the nebular and the sidereal systems are but different parts of one single scheme.

But I pass on to other evidence, independent of what has hitherto been adduced, and pointing with equal force to the same conclusion.

In the northern heavens it is not very easy to exhibit any general law of arrangement associating the nebulæ and the fixed stars. For reasons which yet remain to be detected, there are in fact many marked points of difference between the whole character of the heavens on the northern and on the southern side of the galactic zone. But even in the northern heavens one peculiarity has been remarked, which is well worthy of careful consideration. Sir William Herschel, while prosecuting his series of researches among stars and nebulæ, was struck by the circumstance that, after sweeping over a part of the heavens which was unusually barren, he commonly met with nebulæ; insomuch that it was his practice at such times to call to his assistant (his sister, Miss Caroline Herschel) to "prepare for nebulæ." This peculiarity was noticed also by Sir John Herschel.

Now, what are we to understand by such a relation as this?

Can we suppose that, owing to some strange accident, external galaxies have been placed always opposite the barest regions of the sidereal system? Or, setting aside such a notion as obviously incredible, are we to imagine that, when searching over those barren regions, the astronomer has a better chance of detecting nebulæ than where stars are more richly strewn, because the sky is less filled with glare? We are forced to dismiss this notion, that the barren regions of the heavens are thus in a manner the spy-holes of the sidereal system, by the fact (presently, and for another purpose, to be dwelt on more at length) that in the Magellanic Clouds, where stars of all magnitudes are richly strewn, nebulæ, even down to the very faintest orders, are more abundant than in any other region of the heavens. We have, then, no other conclusion to form, but that the association thus observed between starless regions and richness of nebular distribution indicates a very close relation indeed between stars and nebulæ; that, in fact, *the nebulæ in a sense represent the missing stars; that the region where those nebulæ appear has been drained of star-material, so to speak, in order to form them.*

In the southern heavens yet clearer proof exists of an association between the stellar and nebular systems. We do not recognize in the northern skies any well-marked star-streams. In the southern skies, however, such streams have been recognized from the earliest ages. The constellations Hydra and Eridanus, the two streams from the Water-can of Aquarius, and the band between the two fishes,* indicate how clearly the ancients traced certain well-marked star-streams. The moderns have traced the extension of some of these streams in the constellations Grus, Hydra, Reticulum, etc., into the near neighborhood of the southern pole. Now, the nebulæ in the southern heavens exhibit a well-marked tendency to aggregate into streams. So that, in this mere resemblance between the general characteristics of the stellar and nebular systems in the southern heavens, we have a somewhat remarkable evidence of association. But when we consider the disposition of the two sets of streams — the stellar and the nebular — this evidence is very much strengthened. There is found to be a well-marked correspondence between the nebular and stellar streams, not merely as respects general position, but even in minute details — the nebular streams following the windings of the stellar ones. Such a relation would be very remarkable, even were it observed but in a single instance. Since, however, all the well-marked star-streams in the southern heavens are associated with well-marked nebular streams, no doubt can remain that the relation is not a mere coincidence, but indicates a real association between the nebular and stellar systems.

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* Though Pisces is not a southern constellation, yet it is south of the galactic circle, to which I am for the moment referring the constellations.

But yet more striking evidence remains to be considered.

In the southern heavens there are two strange clouds of milky light, which have long been known by sailors as the Magellanic Clouds, but are commonly called by astronomers the Nubeculæ. Each of these objects, when examined with the telescope, is found to be constituted, like the Milky Way, of multitudes of small stars. But, unlike the Milky Way, the Nubeculæ contain within their bounds many nebulæ of all orders. In fact, each of the Nubeculæ is at once a star-cluster and a cluster of nebulæ.

Now, there can be no doubt whatever that the association here is not accidental, that we do not by some strange chance see a great star-cluster in the same direction as a much more distant and much vaster cluster of external galaxies. Nor, again, can there be any doubt that the generally circular figure of each Nubecula indicates a general approach to the spherical form in the case of each cluster. The probability that by some strange accident a cluster of cylindrical shape* might be so placed as to exhibit to us a circular figure is exceedingly small; but the chance that two such clusters should be presented in so exceptional a manner may be regarded as evanescent. We are compelled, then, to believe that, within the limits of spheres so placed as to subtend a small angle to the eye, stars of all magnitudes between the seventh and the twelfth inclusive are mixed up with the nebulæ of all degrees of resolvability. "Taking the apparent semi-diameter of the Nubecula Major at three degrees," says Sir John Herschel, "and regarding its solid form as, roughly speaking, spherical, its nearest and most remote parts differ in their distance from us by a little more than a tenth part of our distance from its centre." "It must therefore be taken as a demonstrated fact," he adds presently, "that stars of the seventh and eighth magnitude and irresolvable nebulæ may coexist within limits of distance not differing in proportion more than as nine to ten." This demonstrated fact of Sir John Herschel's is the very fact to which I had been led by other considerations, the fact, namely, that the nebulæ are not external galaxies, but intimately associated with the sidereal system of which in fact they form part and parcel. Dr. Whewell, accepting Sir John Herschel's reasoning as conclusive on the point, adopted the same view. Yet Sir John Herschel himself seems, immediately after establishing this noteworthy conclusion, to have been prepared to abandon it, at least as a demonstrated fact, since he says of it only that "it must inspire some degree of caution in admitting *as certain*" facts directly opposed to it. It must not be forgotten, however, that to the clear vision of this great astronomer the association between nebulæ and fixed stars *had* presented itself as a demonstrated fact; that even in the latest editions of his noble work on astronomy, he has not altered the words in which he has spoken of that association; and that so able a reasoner as Dr. Whewell has chosen rather to accept what Herschel has spoken of as a demonstrated fact, than to adopt that measure of caution which Herschel subsequently advocated.

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* Or, more correctly, a cluster shaped like a long frustrum of a gigantic cone.

Lastly, and perhaps most strikingly of all, the association between stars and nebulæ is indicated by the obvious connection between the figure of the irregular nebulæ and the arrangement of the star-groups seen in the same field of view. There is not one of the irregular nebulæ which does not exhibit this peculiarity in the most striking manner. This may be asserted even of those nebulæ with respect to which Sir John Herschel has remarked that the arrangement may be, and probably is, purely accidental. His own pictures prove in the most convincing manner that no such explanation can be accepted. Were the peculiarity confined to the feature Herschel limits his attention to, one might adopt his explanation. The mere aggregation of a large number of stars on the very heart of a nebula might be an accident. The fact, for instance, that the great irregular nebula surrounding the star Eta Argus agrees exactly in position with the greatest condensation of the wonderfully rich portion of the Milky Way on which that surprising variable lies, might be a mere coincidence, though in any case it would be a strange one. But when one examines the structure of this and similar nebulæ, and finds that the stars are arranged in a manner most obviously related to the arrangement of the nebular condensations (or folds as one may almost say), one cannot doubt that a real and intimate bond of association exists between the stars and the nebulous masses around them. If the extension of the milky light of the great Orion nebula to the star in the sword, which is centrally involved in strong nebulosity, to ε in the belt, which is similarly involved, and to several other stars in the constellation all alike in being regions of increased nebular condensation, be a mere accidental coincidence, then the laws of probability had better be forgotten as soon as possible, for, as at present understood, they can only serve to lead men astray.

In the accompanying plate is given a picture of the nebula, Messier 17, as observed with Lassell's four-feet reflector at Malta. I have selected it as affording a very striking instance of the particular form of association I have just been dealing with. No one can, I think, refuse to recognize the fact that the system of stars shown in this drawing is not accidentally seen projected on a distant galaxy, but forms part and parcel of the nebula itself.

It will be noticed, as respects the two proofs on which I have last dwelt, that they seem directly opposed to those which I first quoted. One cannot argue, it might be urged, that the nebulæ are associated with the sidereal system because they are least numerous where there are most stars, and *vice versa;* while at the same time one draws the same conclusion from the aggregation of the nebulæ in streams or clusters where there are streams and clusters of stars, or from the fact that stars are seen actually mixed up with nebulous matter. At first sight this objection seems just; but, on consideration, it will be found that, in reality, the two seemingly contrary lines of argument bear in the same direction. When we find the nebulæ gathered where stars are wanting, and *vice versa,* we conclude that there is some reason for this peculiarity, and that that reason must involve some sort of association between the nebulæ and the stars; we see, further, that the relation is accounted for if we suppose that, in these cases, either the formation of nebulæ has drained a region of material from which single stars would otherwise have been formed, or *vice versa.* Why, in a particular region, the formation of nebulæ should be encouraged, while the formation of stars should be checked, we cannot say; nor can we account for the contrary peculiarity in another region; but we feel certain that some cause must exist for both relations, because the results are too marked to be the result of accident. Now, in the case where we find both stars and nebulæ abundant in particular parts of the heavens, we feel equally certain that the result is not accidental. Even though there were not here, as in the former case, the evidence of a clearing of star-material from certain regions, we could not doubt that the association of stars and nebulæ was real and not apparent. But in reality there is *here,* precisely as in the former case, a gathering together of stellar matter into certain regions. The very existence of such a stream as Eridanus or Hydra, and of such a cluster as the greater or lesser Magellanic Cloud, implies the action of such a process of segregation. A stream would not be recognizable if it were not bounded by relatively bare regions. Clusters like the Nubeculæ *might* be visible even on a rich sky, and were the sidereal heavens richly strewed with stars round these objects I should be disposed to admit that there was a difficulty in my theory. But what is the fact? Not only is each of the Nubeculæ placed in a region obviously bare of lucid stars, but Sir John Herschel, speaking of the telescopic aspect of the neighborhood of these mysterious clusters, dwells again and again on its poverty. " A miserably poor and barren region," he says of one field near the Nubeculæ. "The access to the Nubeculæ," he says elsewhere, " is on all sides through a *desert.*" What evidence could more clearly point to the fact that these great clusters are gathered out from a vast region of space? Their internal structure teaches us how such a process of segregation leads to the birth of nebulæ, as well as stars. The whole history of the sidereal system is indeed taught us in the Magellanic Clouds and the great streams of intermixed stars and nebulæ which flow toward them as rivers toward some mighty lake. We see the wonder-working forces of gravitation extending their influences throughout vast regions of space, gathering in the materials spread throughout that space, here forming stars, there nebulæ, changing the element of distance into various forms of force — heat and light, electricity and magnetism — and finally (though in what special way we are as yet unable to perceive) making the orbs which it has formed the seats of life, or subservient, more or less directly, to the wants of living creatures.

CHAPTER XIII.

SUPERVISION AND CONTROL.

IT HAS been customary, in treatises on the plurality of worlds, to discuss the religious difficulties which seem to suggest themselves when man regards the universe around him as thronged with worlds, each peopled with millions of living creatures, and many perchance the abode of intelligent and therefore responsible beings. Accustomed to regard himself as in a special manner the object of God's care and solicitude, it is not without a sense of pain that he is brought to contemplate the possibility that other creatures may exist in uncounted millions whom God regards with infinite love and interest. "If this be so," asks Whewell, "how shall the earth and men, its inhabitants, *annihilated* as it were by the magnitude of the known universe, continue to be any thing in the regard of Him who embraces all? Least of all, how shall men continue to receive that special, preserving, providential, judicial, personal care, which religion implies; and without the belief in which, any man who has religious thoughts must be disturbed and unhappy, desolate and forsaken?

I do not, however, feel by any means invited to consider "the religious difficulty" by the success which has attended the efforts made by others to remove it. I find that, while on the one hand, the thoughtful and conscientious men who have in a special manner considered the difficulty have been (in relation at least to revealed religion) at issue among each other, their views have not, on the other hand, been found acceptable even by a few among their readers. I doubt almost, when I judge from the comments which have been made on this part of the works of Chalmers, Whewell, Brewster, and others, whether a single reader of those works has found the religious views of any one of their authors congenial with his own.

It is specially noteworthy that even where, as in the case of Brewster and Chalmers, two writers adopt the same view of the general question of other worlds, they yet hold altogether different views as to the bearing of that question upon the subject of religion.

It is very doubtful, therefore, whether it is a wise thing, whether it is conducive to the purpose of any one thus conscientiously discussing the religious aspect of our question, to present his own personal views on the subject of revealed religion. If I thought otherwise, I should not shrink from the task of indicating the sufficiently definite views which I entertain myself upon this subject. But I apprehend that, apart from the consideration that the reader must be wholly indifferent about them, my indicating them would have an effect the very reverse of that which I should desire.*

Merely remarking, therefore, that in considering the infinity of God's beneficence we must remember this quality of infinity, that it comprises many infinities, I pass on to considerations which seem to fall more naturally within the province of the student of science.

It is a peculiarity of the subject of other worlds than ours, that it suggests, more strikingly than any other, certain difficulties in connection with the conceptions we are to form as to the supervision and control exercised by the Creator over His works. We feel that if we are to believe, as we must believe, in an infinitely powerful and wise God, we must not merely regard all the worlds which people space as objects of His regard, but every event, however seemingly insignificant, occurring in any, even the least important of His worlds, as an essential part of the plan according to which all things were created from the beginning.

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* Where Bacon has selected to be silent, few can without presumption venture to lay down their opinions as of weight in matters connected with revealed religion. The argument which follows may not indeed be acceptable to many, but few will doubt the wisdom of the conclusion to which he comes. "If we were disposed," he says, "to survey the realm of sacred or inspired theology, we must quit the small vessel of human reason, and put ourselves on board the ship of the Church, which alone possesses the Divine needle for justly shaping the course. Nor will the stars of philosophy, that have hitherto principally lent their light, be of further service to us; and *therefore it were not improper to be silent upon the subject."*—*Advancement of Learning*, Book IX.

But here already — such is the nature of the subject I am to deal with — I have been forced to use terms which have really no proper application to the Almighty and His works. I have spoken of the creation of all things, whereas, in the sense in which men can alone interpret such words, we cannot reasonably conceive that there ever was a creation; and I have spoken of the beginning, whereas we cannot conceive that there ever was a beginning in the sense implied.*

Let us consider definitely (even though we must be unable to conceive clearly or at all) the infinities we have to deal with.

We know that space must be infinite. If the region amid which stars and nebulæ are scattered with so great profusion be limited, if beyond lies on all sides a vast void, or if, instead, there be material bounds enclosing the universe of worlds on every hand, yet where are the limits of void or bound? Infinity of space, occupied or unoccupied, there must undoubtedly be. Of this infinity it has been finely said, that its centre is everywhere, its boundary nowhere. Now, whether within this infinity of space there be an infinity of matter, is a question which we cannot so certainly answer. Only, if we were to accept *this* as certain, that the proportion which unoccupied bears to occupied space cannot be infinitely great — a view which at least seems reasonable and probable — then it would follow that matter as well as space must be infinite, since any finite proportion of infinity must itself also be infinite. So that, regarding occupied space as the realm over which the Almighty's control is exercised, and over which His supervision extends, we find just reason for looking upon that realm as no less infinite than the infinity of space in which it is contained.

Time also must undoubtedly be infinite. If the portion of time which has hitherto been, or which will hereafter be, occupied with the occurrence of events (of whatever sort) were preceded and will be followed by a vast void interval, yet there can be neither beginning nor end to either of those bounding voids. Infinity of time, occupied or unoccupied, there must undoubtedly be. And, though it is not possible for us to know certainly that there has been no beginning, or that there will be no end to that portion of time which is occupied with the occurrence of *events* (of whatever sort), yet it appears so unreasonable to conceive that unoccupied time bears an infinitely great proportion to occupied time, that we seem forced to the conclusion that occupied time is infinite — or, more definitely, that there has been no beginning and will be no end to the sequence of events throughout the infinitely-extended realm of the Almighty.

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* To prevent any possibility of my meaning being misinterpreted here, I point out that I have been obliged myself to use the terms of which I speak as inexact.

And thus we are forced to believe in the infinite wisdom and the infinite power of God; since to conceive of limits to the wisdom and power of Him whose realm is infinite in extent and in duration, is obviously to conclude that the Ruler is infinitely incompetent to rule over His kingdom; for there can be no relation between the finite and the infinite save the relation of infinite disproportion.

Now, although the conception of God as a spirit — omnipresent, eternal, omnipotent, and omniscient — is altogether beyond the powers of man's imagination, yet we may consider certain relations between the way in which He views the universe and the modes in which we men consider the various matters falling either under our supervision and partial control, or of which we can in any way or to any extent become cognizant.

Senses such as we have we can no more attribute to God than we can assign to Him hands and feet. Nor can we conceive in what way a spirit, as He is, is cognizant of material processes which we only recognize through their material effects. Yet, as we do not doubt that God is cognizant of the actual state of the universe at any moment, we cannot doubt that He is cognizant of all those processes by which our senses can be affected. And clearly, He not only recognizes all these processes in such sort that He may be said to see what we see, to hear what we hear, and so on; but effects which, though related to vision, hearing, or the like, are infinitely too minute to be appreciated by our senses, must be as obvious to God as the light of day or the roar of thunder to ourselves.

But, before considering the nature of God's supervision of His universe, we may proceed a step farther. The senses we possess are sufficient to indicate to us the possible existence of senses not merely far more acute, but of a wholly different kind. By the sense of touch, for instance, we can indeed recognize the feeling of heat; but it is easy to conceive of a sense (analogous to that by which light is made to teach us of the aspect of external objects) enabling men to judge of the figure, substance, internal structure, and other qualities of an object, by the action of the heat-waves proceeding from it. Or again electricity might, instead either of light or of heat, be the means of communicating intelligence as to the qualities of objects. We can conceive also of a sense bearing the same analogy to sight that the spectroscope bears to the telescope. And a hundred kinds of sense, or in other words, a hundred modes of receiving intelligence about what exists or is going on around us, might be readily conceived. Now, we cannot doubt that the natural processes involved in every such mode of conveying impressions to material creatures must be infinitely more obvious to God than we can possibly conceive them to be to material beings.

Yet once more, we know that reason is able to range beyond the action of the senses. Man is able to assure himself that events have happened which yet have produced no direct effect upon any of his senses. By the exercise of reason he becomes as well assured of such events as though they had actually passed before his eyes. We must assume that an analogous power, but infinite in degree, infinitely rapid in its operation, and infinite in the extent of space and time over which it ranges, is possessed by the Almighty.

And now let us notice some of the conclusions to which these considerations tend.

Let us first deal with the teachings of that sense, which is the most far-reaching* of all the faculties given to man — the sense of sight.

In a little treatise called "The Stars and the Earth," published anonymously several years since, some results of modern discoveries respecting light were dealt with in a very interesting manner. I propose to follow the path of thought indicated in that treatise, as a fitting introduction to wider conceptions of the supervision and control exercised by the Almighty over His universe.

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* Most persons, if asked which sense comes next to sight in this respect, would answer hearing. Yet *touch* — or rather *feeling* — has a range far exceeding that of hearing, since we can feel the heat emitted by the sun. Nor is it difficult to conceive of such an increase in the delicacy of the sense of touch, that even the minute amount of heat received from the fixed stars might be felt, and so the range of the sense extended many million-fold.

We know from Römer's researches, and even more surely from the phenomenon termed the aberration of the fixed stars, that light does not travel with infinite velocity. Its speed is indeed so enormous, that, compared with every form of motion with which we are familiar, the velocity of light appears infinitely great. In a single second light traverses a space equal to eight times the circumference of the earth; and therefore, in travelling from any visible object on the earth to the eye of a terrestrial observer, light occupies a space of time indefinitely short. Yet, even as regards such objects as these, light has occupied a real interval of time, however minute, in reaching the eye; insomuch that we see objects not as they are at the moment we perceive them, but as they were the minutest fraction of a second before.

Raising our eyes from the earth to regard the celestial objects, we find, in place of the indefinitely minute interval before considered, a really appreciable space of time occupied by light in carrying to us information as to the condition of those distant orbs. From the moon, light takes little more than a second and a quarter in reaching us, so that we obtain sufficiently early information of the condition of our satellite. But light occupies more than eight minutes in reaching us from the sun, a longer or shorter interval in travelling to us from Mercury, Venus, and Mars, according to the position of these planets, from about thirty-five to about fifty minutes in reaching us from Jupiter, about an hour and twenty minutes on the average in speeding across the great gap which separates us from Saturn, while we receive intelligence from Uranus and Neptune only after intervals respectively twice and three times as great as that which light takes in reaching us from the ringed planet.

Thus, if we could at any instant view the whole range of the solar system as distinctly as we see Jupiter or Mars when in opposition, the scene presented to us would not indicate the real aspect of the solar system at that, or indeed at any definite instant. Precisely as a daily newspaper gives us a later account of what is going on in London than of events happening in the provinces, of these than of events on the Continent, and of these again than of occurrences taking place in America, Asia, Africa, or Australasia, so the intelligence brought by light respecting the various members of the solar system belongs to different epochs. And if man had powers of vision enabling him to watch what is taking place on the different planets of the solar system, it is clear that events of the utmost importance might have transpired — under his very eyes, so to speak — while yet he remained wholly unconscious of their recurrence. Or, to invert the illustration, if an observer on Neptune could see all that is taking place on the earth, he might remain for hours quite unconscious of an event important enough to affect the welfare of a whole continent, though that event should happen under his eyes, and his visual powers be such as I have supposed. We can imagine, for example, an observer on Neptune watching the battle of Waterloo from the early dawn until the hour when Napoleon's heart was yet full of hope, and our great captain was watching with ever-growing anxiety, as charge after charge threatened to destroy the squares on whose steadfastness depended the fate of a continent. We can conceive how full of interest that scene would have been to an intelligent Neptunian, and how eagerly he would have watched the manœuvres of either army and also, what neither army knew of, the approach of Blucher with his Prussians. Yet, while our Neptunian would thus have traced the progress of the battle from his distant world, the conflict would in reality have been long since decided, the final charge of the British army accomplished, the Imperial Guard destroyed, Napoleon fugitive, and the Prussians, who to the Neptunian would be seen still struggling through muddy roads toward the field of battle, would be relentlessly pursuing the scattered army of France.

It is, however, when we pass beyond the limits of the solar system that the non-contemporaneous nature of the scene presented to us becomes most striking. Here we have to deal not with seconds, minutes, or hours, but with years, decades, and centuries. From the nearest of the fixed stars light takes fully three years in travelling to the earth. Even the star 61 Cygni is so far from us that its light only reaches us in ten years. And, so far as observation has hitherto gone, it seems unlikely that, amid the whole host of heaven, there are so many as a hundred stars — lucid or telescopic — whose light reaches us in a shorter interval of time than twelve or fifteen years. Whatever views we form as to the arrangement of the sidereal scheme, whether those usually accepted be held to be correct, or whether I have been right in adopting others, there can be no doubt that, among the stars revealed to us by the telescope, there must be myriads which lie many times farther from us than the bright star in Centaurus and the orb in Cygnus which have been found relatively so near to us. In fact, the views I have adopted, respecting the wide range of magnitude among the fixed stars, do not interfere in the least with the theories which have been formed as to the distances from beyond which the light of some of the stars, only just visible in powerful telescopes, must be supposed to reach us. On the contrary, one may conceive, according to my views, that some of these faintly-seen orbs may be many times larger even than giant Sirius, in which case the distance of such stars would be many times greater than has been hitherto supposed. We may at any rate assume with confidence that many stars only visible in powerful telescopes shine from beyond depths which light would occupy thousands of years in traversing. I cannot, indeed, go further, as astronomers have hitherto done, and say that the nebulæ must be regarded as external galaxies, and therefore as sending their light to us over spaces which light must take many times as long an interval in traversing as it does in travelling to us from the bounds of our own galaxy. But it would be to misinterpret altogether the views which I have formed respecting the universe to suppose that I imagine those distant spaces which astronomers have hitherto filled with imaginary galaxies to be untenanted. On the contrary, I have no doubt whatever that galaxies, resembling our own, exist at distances infinitely exceeding those at which astronomers have placed their most distant nebular universes, if even the bounds of our own galaxy do not extend into space as far as the widest limits hitherto assigned to the system of nebulæ. So that I am not precluded from speaking of orbs whose light, though unrecognized by us, yet is ever pouring in upon the earth, conveying, though in letters we cannot decipher, or even trace, a message which has taken millions on millions of years in traversing the awful gulf beyond which lie those mysterious realms.

If we conceive, then, that man's visual powers could suddenly be so increased that, without instrumental aid, he could look around him into the celestial depths, piercing even to those outer galaxies which astronomers have seen only imaged in the nebulæ, how wide would be the range of time presented to him by the wonderful scene he would behold. There would blaze out Alpha Centauri with its record three years old; there the star in Cygnus as it existed ten years since; the whole host of stars known to man would exhibit records ranging from a few years to many centuries in age; and, lastly, the external galaxies, which are perhaps forever hidden from the searching gaze of man, would reveal themselves as they were ages on ages before man appeared upon the earth, ages even before this earth was framed into a globe, nay ages perhaps before the planetary system had begun to gather into worlds around its central orb.

It is when we are thus contemplating in imagination the whole expanse of the universe, and, as one may almost say, the whole range of past time, that the author of the little treatise I have spoken of invites us to consider two processes of thought having sole reference to this earth on which we live, and to that history which, though all-important to ourselves, seems to fade into such utter insignificance in the presence of the grand history of the orbs which lie in uncounted millions around us.

To a being placed on some far-distant orb, whence light would occupy thousands of years to wing its flight to us, there would be presented, if he turned his gaze upon our earth, and if his vision were adequate to tell him of her aspect, the picture of events which thousands of years since really occurred upon her surface. For the light which left the earth at that time, winging its way through space with the account, if we may so speak, of those occurrences, is now travelling as swiftly as when it left our earth, but amid regions of space removed from us by a light-journey thousands of years in duration. And thus, to the observer on this distant orb, the events which happened in those far-off years would seem to be actually in progress.

But now conceive that powers of locomotion commensurate with his wonderful powers of vision were given to this being, and that in an instant of time he could sweep through the enormous interval separating him from our earth, until he were no farther from us than the moon. At the beginning of that tremendous journey he would be watching events which were occurring thousands of years since; at its close he would gaze upon the earth as it was one second only before he undertook his instantaneous flight; so that, in the course of his journey, he would gaze upon a succession of events which had occurred during those thousands of years upon the face of this little earth.

The other conception is no less beautiful and striking — I may remark, also, that it is, in a scientific sense, somewhat more exact. Suppose that a being armed with such powers of vision as we have imagined should watch from the neighborhood of our earth the progress of some interesting event. If he then began to travel from the earth at a rate equal to that at which light travels, he would see one phase of the event continually present before him, because he would always be where the light-message recording that event was actually travelling. By passing somewhat less swiftly away, he would see the event taking place with singular slowness; while by passing away more swiftly he would see the event occurring in inverted order. Suppose, for example, he were watching the battle of Waterloo, he could gaze on the fine picture presented by the Imperial Guard as they advanced upon the English army, for hours, years, nay, for centuries or cycles; or he might watch the whole progress of the charge occurring so slowly that years might elapse between each step of the advancing column, and the bullets which mowed down their ranks might either seem unmoving, or else appear to wend their way with scarcely perceptible motion through the air; or, finally, he might so wing his flight through space that the Guard would seem to retreat, their dead men coming to life as the bullets passed from their wounds, until at length the Old Guard would be seen as it was when it began its advance, in the assured hope of deciding Waterloo, as it had decided so many hard-fought battles for its imperial chief.

It may seem hypercritical to notice scientific inexactness in ideas professedly fanciful. But as the author lays some little stress upon the scientific truth of the method in which his fancies are exhibited, and as, further, he dwells upon two of the more obvious objections to the first conception, it may be well to consider a further objection, which enforces on us a total change in the way of presenting the idea. He remarks that the being he has conceived to be borne toward the earth through a distance so enormous, would not see in a moment the whole history of the earth during the thousands of years considered, but only the history of that hemisphere which was turned toward him; while, further, all that took place under the roofs or under cover of any sort, would remain unperceived by him. But there is a more serious objection. Among the events which have taken place during those thousands of years, have been thousands of revolutions of this earth around the sun, and more than three hundred and sixty-five times as many rotations of this earth upon her axis, to say nothing of the stately sway of the earth in her motion of precession. So that our imaginary observer would in reality see the earth whirling with inconceivable rapidity upon its axis, and sweeping with even more tremendous velocity around the sun, so as to complete thousands of circuits in a single second. He would see clouds forming and vanishing in an amazing succession of changes, all occurring in a single instant. And, even though his powers of vision enabled him to pierce the cloud-envelope, he would not have a consecutive presentment of the various events occurring in any part of the earth, but only a hap-hazard succession of half days for each portion of her surface.

However, we can easily see that, by a slight modification, the beautiful conception of our author can be made to illustrate one mode at least in which the events occurring upon our earth may be conceived to be at all times present to the thoughts of the Almighty. Imagine a sphere with a radius over which light would travel in the time which has elapsed since living creatures first began to move upon this earth, and having for centre the place occupied by the earth at that instant. Then, if we imagine millions of eyes over the surface of that sphere, all turned with piercing powers of vision upon the central earth, we see that to these eyes the earth would be presented by the record of light, not as she is now, but as she was at that primeval day. Now, conceive these millions of eyes closing swiftly in upon the earth, but with this peculiarity of movement that, instead of being always on a sphere around a fixed point, they were always on a sphere around the position which was really occupied by the earth, when the light-messages started which those eyes are receiving at the moment. Then if that wondrous sphere contracted in an instant, according to the law assigned it, until its myriad millions of eyes were gazing intently on our earth from a sphere of but a few thousand miles in radius, the whole history of the earth, so far as light could render it, would have been in a moment of time presented before the myriad-eyed sphere.

To apply this illustration to the subject we are upon. We know that the Almighty is present where the boundary of our great sphere was placed at first. Before Him the light-messages are presenting the account of the primeval earth. He also is present everywhere within the region through which the contracting sphere was conceived to pass. He therefore sees the whole history of the earth as presented by the light-waves. We begin, however, already to feel that we cannot say of Him what we said of the imaginary being first thought of, or of the myriad-eyed contracting sphere, that in a moment of time He can see the whole history of the earth *successively* presented before Him. As He exists throughout that space, there is no succession of time in His vision of the events transpiring on our globe. Past and present are one before Him; and we shall soon see that present and future also must be one in His sight.

But now, still considering only the information which light conveys as it travels onward through space, we see that what is true of our earth is true also of every orb throughout the universe. The whole light-history of every such orb must be present at every instant of time to the Creator who is omnipresent. So that to the obvious conception that God, being everywhere, must be cognizant of all things, we have to add this further consequence of His omnipresence, that He must be cognizant of the history of all things, in the same sense that a man is cognizant of events which are passing before his eyes.

And, by extending these considerations to other modes in which the history of an event is recorded, so to speak, by natural processes, we can see that a much more complete and definite picture of past events than light can convey, must be at all times present before the Almighty. A sense which could analyze heat-impressions as eyesight analyzes light, would tell us not only what eyesight tells us, but much that no light-messages can convey to us. At least it is conceivable that a sense of this sort would enable the being provided with it to recognize not merely the nature of the surface of any body whose heat reached the organ of this sense, but the quality of the body's internal structure, processes going on within the body, or the nature of bodies so placed that eyesight would not render us sensible even of their existence. Electricity, in like manner, would avail to give information altogether distinct from that which light can impart. And precisely as, in considering light, we saw that the Creator must be supposed sensible of every light-record travelling through space, so, as regards these imaginary but conceivable senses, we must believe that any information which they could by any possibility impart, must be conveyed to the omnipresent God. And further, it would be a contradiction to our belief in His infinite wisdom to suppose that the infinite multiplicity of the records thus continually present before Him could in any way render their significance less distinct.*

But, turning from the consideration that the Almighty, by virtue of His omnipresence, is thus cognizant, not merely of all that is at any moment taking place throughout the universe, but of all that has taken place in the infinity of past time, we have to consider another mode in which the universe must be regarded as present before Almighty God.

The senses by which we judge of what is going on around us are, after all, merely one means by which we judge of causes by their effects. When we say, for instance, that we have seen such and such an object, or watched such and such an event, what we really imply is, that we have recognized certain physical impressions, which we can only explain by the existence of that object, or by the occurrence of that event. We know, in fact, that in certain exceptional cases impressions resembling those caused by the actual presence of an object, or by the actual occurrence of some event, may arise where no such object has been present, or where no such event has transpired. Still, we commonly feel safe from error, in concluding, from certain impressions conveyed to the mind by the agency of the visual organs, that certain objects have been really present, at rest or in action, before us.

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* Moralizing may seem altogether out of place in such a work as this, but certainly one is tempted to dwell somewhat thoughtfully on the ideas raised by the considerations I have dwelt on above. It is not without a feeling of awe that one considers that the records of every action of our lives are not merely at this moment before God, but will for ever and ever be freshly present to Him: and *that*, not merely in the sense that He knows every thing (an idea too vague for man rightly to grasp), but by the action of physical processes such as our Faradays and Tyndalls deal with. May it not be through an instinctive recognition of this great truth, that man alone, of all the creatures which people this earth, feels contrition for long-past misdeeds, even where he has no fear of their ever bearing fruit in future sorrows?

But, then, even man, limited as are his powers, can yet follow a series of effects and causes far more numerous than those concerned in the act of vision; and so he can become certain of the occurrence of past events of which no sense he possesses gives him any direct information. For example, though I neither saw the battle of Waterloo nor heard the thunder of the guns there, yet I am as certain that the battle really took place as though sight and hearing had given me direct information on the matter. And, when I inquire whence that certainty arises, I find a complicated series of events involved in my acquisition of the knowledge that the battle took place. My interpretation of the letter-press account of the battle involved in itself a number of more or less complex relations, associated with the question of my confidence in those who taught me that certain symbols represented certain letters, that certain combinations of letters represented certain words, and that certain words represented certain ideas. Not to follow out the long train of ideas thus suggested, it will be clear that, with regard to a variety of matters, the knowledge which any man has is associated with considerations of cause and effect, of general experience, of confidence in the accounts of others or in his own judgment, which are in reality of a highly-complex character.

Now, we are led by these thoughts to remember that, independently of those records of past events which are brought continually before the Almighty by processes resembling those which directly affect our senses, such events must be recognizable by Him (even to their minutest details) in the consequences which they have led to. If a great naturalist like Huxley or Owen can tell, by examining the tooth of a creature belonging to some long-extinct race, not only what the characteristics of that race were, but the general nature of the scenery amid which such creatures lived, we see at once that a single grain of sand or drop of water must convey to the Omniscient the history of the whole world of which it forms part. Nay, why should we pause here? The history of that world is in truth bound up so intimately with the history of the universe, that the grain of sand or drop of water conveys not only the history of a world, but with equal completeness the history of the whole universe.

The Almighty, then, by virtue of His possessing in an infinite degree that quality which enables man to reason upon past events of which his senses bring him no direct intelligence, has the whole past history of the universe continually present before Him, in the state and position of each single atom throughout infinity of space.*

Turning from the past to the future, we must not let the limited nature of our recognition of the course of future events prevent us from forming a just opinion as to the way in which the future must be always present before God. We can judge of the past by its effects, but we are almost utterly unable to judge of the future by its causes. Yet we cannot doubt that the future *is* present in its germs, precisely as the past is present in its fruits. It may be regarded in fact as merely a peculiarity of man's constitution that the past is more clearly present to his mental vision than the future. It is easy not only to conceive that the future and the past should be equally present to intelligent creatures, but to conceive of a form of intelligence according to which past events would be obliterated from the mind as fast as they took place, while the future should be as actually present as to the ordinary human mind the past is.

In considering the Omniscient, however, all questions of degree must be set on one side. The future must be as absolutely and essentially present to Him in its germs as the past has been shown to be in fruits. If a grain of sand contains in its state, figure, and position, the picture of the universe as it is, and the whole history of the universe throughout the infinite past — and who can doubt that this is so? — it contains with equal completeness the history of the universe throughout the infinite future. No other view is compatible with the assumption of the Almighty's infinite wisdom, and no assumption which limits the wisdom of God is compatible with our belief that He is supreme in the universe.

Obviously also every *event,* however, trifling, must be held to contain in itself the whole history of the universe throughout the infinite past, and throughout the infinite future. For every event, let its direct importance be what it may, is indissolubly bound up with events preceding, accompanying, and following it, in endless series of causation, interaction, and effect.

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* In fact, if we consider the matter attentively, we see that there cannot be a single atom throughout space which could have attained its present exact position and state, had the history of any part of the universe, however insignificant, been otherwise than it has actually been, in even the minutest degree.

So far, then, as the Almighty's watch over His universe is concerned, we have two lines of thought, each leading to the recognition of a perfect supervision. In virtue (1) of His omnipresence, and (2) of His infinite wisdom, He sees at each instant the whole universe as it has been in the infinite past, as it is now, and as it will be in the infinite future; and this being as true of any one instant as it is of any other, we recognize the operation of yet a new form of infinity — the infinite duration of the Almighty's existence — to render yet more inconceivably perfect God's supervision of His universe.

And now with regard to control. Does the Almighty, who supervises all things, exercise any controlling action upon the course of events?

It need hardly be said that, if God does exercise control, apart from the laws which He has assigned to His universe, His knowledge of the progress of past and future events is not therefore to be called in question, since His own direct action, whether in the past or in the future, is quite as much the subject of His consciousness (to use this word for want of a better) as the action of His creatures or of the laws He has primarily set them.

We know that certain laws have been assigned to the universe, and we know also that, so far as our very limited experience enables us to determine, these laws are never abrogated.* Here

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* All things working thus according to law, however, certain difficulties suggest themselves which must not be left undealt with, since not to consider them might be to leave painful doubts in the minds of some who may read these pages.

In the first place, there is the old question of the relation between man's free-will and the absolute foreknowledge of Almighty God. It seems clear to many that if all things are foreknown there can be no such thing as free-will; insomuch that some have even felt forced to believe that the Almighty, though undoubtedly omniscient, must in a sense forego His knowledge of future events so that the actions of men may be subject to the control of their will. But in reality we have only to consider the analogy of human foreknowledge, to see that there is no necessity for any theory so self-contradictory as this. We have already considered other attributes of the Almighty as in a sense resembling, though infinitely exceeding in range of action, certain attributes of man; let us, then, inquire whether that attribute of man which, though imperfect and limited, yet corresponds to the foreknowledge of God, affords us any reason for believing that perfect foreknowledge bars the exercise of free-will. The answer is obvious at once. We know

I set altogether aside, for the moment, the possibility of miracles, and consider only the results of experimental or observational science. Thus, we are led to the conclusion that all

that we often judge, with more or less certainty of conviction, that such and such acts will be performed by others, and that yet our anticipation in no sense influences the will of the persons who are expected so to act. Suppose I remember, for example, that I have left a valuable in a room which will presently be passed through by one whom I know to be dishonest; I judge accordingly that the person will purloin the valuable. In this case his free-will is not affected by my anticipation; nor would it be though a yet clearer conviction of his conduct were impressed upon me. There is, in fact, no conceivable degree of certainty on my part which would render him undeserving of punishment for stealing the valuable. And so, not to give further instances where the matter is so obvious, we see that no conceivable degree of foreknowledge bars free-will. The infinite and absolute foreknowledge of the Almighty is therefore altogether dissociated from the dangerous and hurtful belief in a predestination which renders man irresponsible for his actions.

Secondly, the belief in the absolute perfection of the laws according to which God rules His universe, insomuch that throughout all the worlds in space all things work according to those laws without need of special interference on His part, has been thought by many, and is painfully felt by some, to oppose itself to our belief in the efficacy of prayer. In touching on this point, I wish very carefully to avoid any intrusion on matters apart from the general scope of my subject; but a few words may be permitted me on a point which comes close home to the hearts of all of us, and which also does seem in a sense associated with the matters I have been dealing with. All men, I suppose, pray; though many may in words deride prayer, and though hundreds, without expressing doubts, may fail to see any possible utility in the practice, because they cannot believe that the action of the physical laws of God can be interfered with in answer to the appeal of His creatures. It is because I fear lest some of my readers should have felt this difficulty, and should find their doubts confirmed by any thing I may here have written, that I indicate the explanation which I suppose every one who thinks much upon this point would probably be led to. Remembering that, on the one hand, it is unreasonable to conceive that God would have allowed a belief in the efficacy of prayer to grow, as it has done, to be a part of human nature, were that belief founded in a monstrous fiction, and that on the other hand, it is unreasonable to suppose that physical laws are interfered with in response to the millions of prayers daily offered by men, the obvious conclusion seems to be that prayers are responded to (where it has seemed fitting that they should be) without interference with natural laws; that, in fact, the scheme of the Almighty includes at once the prayers and their response. It seems baffling, indeed, to human thoughts that such an infinity of varied interests should thus be provided for, in a scheme whose extent covers the infinity of past and future time; but where infinite wisdom is in question this consideration need not trouble us. Nor is this particular mode of control inconsistent even with our merely human conceptions of what is reasonable. For instance, a father, desirous at once of testing and rewarding the obedience of a son, might tell him to go to such a place and to open such a box, having beforehand placed therein a reward for his son's obedience. Here the fulfilment of the father's request would no more result in bringing the gift to the box, than our fulfilment of the duty of prayer can cause the laws of Nature to cease or change in their operation; yet obedience would in the one case, as we can conceive it does in the other, in reality bring about its own reward. And, further, it may be remarked that, precisely as the greater or less certainty of the father as to his son's obedience would in no sense affect things happen according to set physical laws; and without, by any means, adopting the view that the Almighty exercises no special control over His universe, we see strong reason to believe

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the latter's merit, so neither does the absolute foreknowledge of God as to the prayers which His creatures will offer up, affect in any sense the merit which He has been pleased to recognize in the sincere performance of the act of prayer.

Lastly, there is the difficulty as to our belief in miracles — that is, in events which involve the temporary suspension or alteration of natural laws. It must be remembered here that recent physical researches, though they have enabled us to interpret so many of the laws of Nature, yet tell neither for nor against our recognition of the possibility of miracles. It belongs to the very essence of a miracle that it should be an event which no physical researches can explain, or indeed can affect otherwise than to render it the more inexplicable. The question is, not whether such and such an event is more or less wonderful to the unlearned Hodge or Styles on the one part, or to a Newton or a Faraday on the other, but whether an event can really take place in which the laws of Nature have absolutely been annulled and abrogated. I take it, for instance, that if we could see a hungry multitude fed with a few loaves, and were absolutely certain that so many thousands had been satisfied with what would naturally be the food but of a few, our wonder would not be greater or less whether we viewed the matter as a laborer would, who simply knows what hunger is and what is needed to satisfy it, or whether we were familiar with the analysis of bread and compared the amount of fibrine and albumen contained in the loaves with what we knew of the daily or hourly exhaustion of the corresponding materials in the human frame.

The arguments in favor of miracles or against their having occurred (of their possibility there need be no question) are the same now as they were in less scientific ages. Those who believe in the occurrence of miracles argue thus: Man differs from all other terrestrial creatures in being responsible to his Creator. Thus between him and Almighty God there is a direct relation, which renders it necessary that the will of God should be communicated to man. Now, we can conceive no way in which such communication can be made in an unmistakable manner, but by events which involve an unmistakable exercise of a power belonging to God alone — that is, by vents of a supernatural character. The believer in miracles further argues that nothing tending to prove the impossibility (in a natural sense) of an event of this sort can be accepted to disprove its occurrence, since what is essentially requisite to the very purpose of a miracle is that it should be in a natural sense impossible. Nor is it necessary that any recorded miracle should be in itself of a striking or imposing character, so long as its connection with the communication of God's will in a special manner is reasonably established, since the triviality or non-triviality of an event whose miraculous character is in question is to be judged only by the circumstances of those for whose instruction the miracle is supposed to have been worked.

The argument against the occurrence of miracles has been already considered. As has been pointed out, it not only does not meet the argument just stated, but rests on the very fact which constitutes the basis of that argument — the fact, namely, that the occurrence of miracles is contrary to experience. It is obvious, then, that the considerations I have urged, as to the nature of God's control over His universe, need not be regarded as in the slightest degree affecting the belief of men in those direct relations between God and man which have been held to involve the necessity of miracles. To speak further, however, on this matter would bring me to deal with that subject which I have selected to avoid.

that the laws which He has assigned to it are sufficient for the control of all things. Indeed, so far as all things take place in accordance with laws which the Almighty must assuredly have Himself ordained, we may say that every event which has happened or will happen throughout infinite time is the direct work, and indicates the direct purpose and will, of Almighty God. Nor need the thought that the Almighty thus seems to be made the author of evil as well as of good in any way startle us, because we know that what constitutes evil or good in our limited vision may by no means be accepted as indicative of what is evil or good as the work of God. *We* know, limited as our wisdom is, that evil often works to good, so that if the Almighty, whose wisdom extends over the never-ending chain of sequent events, seems, by permitting evil, to, in a sense, countenance it, we are to recognize the sequent good as in truth His work, and to regard that which is objectively evil (and actually evil in the creature who does it) as subjectively good in Him who permits it with a perfect knowledge of all that in the infinity of future time is to flow from it.

Now, it seems conceivable that in reality it is only our limited acquaintance with the operation of the laws which God has set His universe, which makes us regard them as unchanging, and, so to speak, inexorable. There seems, indeed, reason rather to expect than to deny, that He who made the laws may annul or suspend them at His pleasure.

But I think that this view — though it has been entertained by many thoughtful men, especially because it seems to give the Almighty a special controlling power over His universe — is in reality inconsistent with just conceptions of His infinite wisdom. If His wisdom, though inconceivably great, were yet finite, we could not suppose that the universe would have been so planned (still to use inexact words for want of better), and laws of such a nature assigned to it, that throughout the infinity of time all things should work out the will and purpose of Almighty God. There would then, undoubtedly, be continual need of adaptation, change, remodelling — of the annulment of a law here, or its suspension there — in order that the whole might not fall to wrack. But where the God of Nature is infinitely wise, there can be no such necessity. The whole scheme of the universe must needs be so perfect that direct intervention cannot at any time be required.

To sum up, we find ourselves led to the belief that, while intervention with the operation of natural laws is unnecessary, all the worlds existing throughout space are, in a very definite and special manner, watched over and controlled by an omnipresent, omnipotent, and omniscient Being; that before Him the infinite past and the infinite future of the universe are at all times sensibly present; that each the minutest atom and every the least important event exhibits before Him at each instant the perfect history of the limitless past and future of the universe; and lastly, that His infinitely perfect consciousness of, and control over, all that has been, is, or will be, are infinitely multiplied (to use the only available expression) by the infinite duration throughout which His existence extends.

THE END.

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LIGHT SCIENCE FOR LEISURE HOURS

A SERIES OF FAMILIAR ESSAYS ON SCIENTIFIC
SUBJECTS, NATURAL PHENOMENA, ETC.

BY

Richard A. Proctor

B. A. CAMB., F. R. A. S.

AUTHOR OF "THE SUN," "OTHER WORLDS THAN OURS," "SATURN," ETC.

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PREFACE.

The Essays in the present volume have been selected from my contributions to serial literature. Although I have for some time been urged to publish such a volume, I think I should not have ventured to do so but for the kindness with which my "Other Worlds" and "The Sun" have been received, both by the press and the public.

In preparing these Essays, my chief object has been to present scientific truths in a light and readable form — clearly and simply, but with an exact adherence to the facts as I see them. I have followed — here and always — the rule of trying to explain my meaning precisely as I should wish others to explain, to myself, matters with which I was unfamiliar. Hence I have avoided that excessive simplicity which some seem to consider absolutely essential in scientific essays intended for general perusal, but which is often even more perplexing than a too technical style. The chief rule I have followed, in order to make my descriptions clear, has been to endeavor to make each sentence bear one meaning, and one only. Speaking as a reader, and especially as a reader of scientific books, I venture to express an earnest wish that this simple rule were never infringed, even to meet the requirements of style.

It will hardly be necessary to mention that several of the shorter Essays are rather intended to amuse than to instruct.

RICHARD A. PROCTOR.

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LIGHT SCIENCE
FOR LEISURE HOURS.

STRANGE DISCOVERIES RESPECTING THE AURORA.

ONE of the most mysterious and beautiful of Nature's manifestations promises soon to disclose its secret. The brilliant streamers of colored light which wave at certain seasons over the heavens have long since been recognized as among the most singular and impressive of all the phenomena which the skies present to our view. There is something surpassingly beautiful in the appearance of the true "auroral curtain." Fringed with colored streamers, it waves to and fro as though shaken by some unseen hand. Then from end to end there pass a succession of undulations, the folds of the curtain interwrapping and forming a series of graceful curves. Suddenly, and as by magic, there succeeds a perfect stillness, as though the unseen power which had been displaying the varied beauties of the auroral curtain were resting for a moment. But even while the motion of the curtain is stilled we see its light mysteriously waxing and waning. Then as we gaze, fresh waves of disturbance traverse the magic canopy. Startling coruscations add splendor to the scene, while the noble span of the auroral arch from which the waving curtain seems to depend, gives a grandeur to the spectacle which no words can adequately describe. Gradually, however, the celestial fires which have illuminated the gorgeous arch seem to die out. The luminous zone breaks up. The scene of the display becomes covered with scattered streaks and patches of ashen gray light, which hang like clouds over the northern heavens. Then these in turn disappear, and nothing remains of the brilliant spectacle but a dark smoke-like segment on the horizon.

Such is the aurora as seen in arctic or antarctic regions, where the phenomenon appears in its fullest beauty. Even in our own latitudes, however, strikingly beautiful auroral displays may sometimes be witnessed. Yet those who have seen the spectacle presented near the true home of the aurora, recognize in other auroras a want of the fulness and splendor of color which form the most striking features of the arctic and antarctic auroral curtains.

Hitherto the nature of the aurora has been a mystery to men of science; nor, indeed, does the discovery we are about to describe throw even now full light on the character of the phenomenon. That discovery, however, affords promise of a speedy solution of the perplexing problems presented by auroral displays; and in itself, it is so full of interest and so suggestive, that our physicists already recognize it as one of the most important which have been made in recent times.

A few brief words in explanation of the progress which had been effected in the study of auroral phenomena, will serve to render the interest and importance of the discovery we have to describe more apparent.

Let it be premised, then, that physicists had long since recognized in the aurora a phenomenon of more than local, of more even than terrestrial, significance. They had learned to associate it with relations which affect the whole planetary scheme. Let us inquire how this had come about.

So long as men merely studied the appearances presented by the aurora, so long in fact as they merely regarded the phenomenon as a local display, they could form no adequate conception of its importance. The circumstance which first revealed something of the true character of the aurora was one which seemed to promise little.

Arago was engaged in watching from day to day, and from year to year, the vibrations of the magnetic needle in the Paris Observatory. He traced the slow progress of the needle to its extreme westerly variation, and watched its course as it began to retrace its way toward the true north. He discovered the minute vibration which the needle makes each day across its mean position. He noticed that this vibration is variable in extent; and so he was led to watch it more closely. Thus he had occasion to observe more attentively than had yet been done the sudden irregularities which occasionally characterize the daily movements of the needle.

All this seems to have nothing to do with the auroral streamers; but we now reach the important discovery which rewarded Arago's patient watchfulness.

In January, 1819, he published a statement to the effect that the sudden changes of the magnetic needle are often associated with the occurrence of an aurora. I give the statement in his own words, as translated by General Sabine: "Auroras ought to be placed in the first rank among the causes which sometimes disturb the regular march of the diurnal changes of the magnetic needle. These do not, even in summer, exceed a quarter of a degree, but when an aurora appears, the magnetic needle is often seen to move in a few instants over several degrees." "During an aurora," he adds, "one often sees in the northern region of the heavens luminous streamers of different colors shoot from all points of the horizon. The point in the sky to which these streamers converge is precisely the point to which a magnetized needle suspended by its centre of gravity directs itself. . . . It has, moreover, been shown that the concentric circular segments, almost similar in form to the rainbow, which are usually seen previous to the appearance of the luminous streamers, have their two extremities resting on two parts of the horizon which are equally distant from the direction toward which the needle turns; and the summit of each arc lies exactly in that direction. *From all this it appears, incontestably, that there is an intimate connection between the causes of auroras and those of terrestrial magnetism.*"

This strange hypothesis was, at first, much opposed by scientific men. Among others the late Sir David Brewster pointed out a variety of objections, some of which appeared at first sight of great force. Thus, he remarked that magnetic disturbances of the most remarkable character have often been observed when no aurora has been visible; and he noticed certain peculiarities in the auroras observed near the polar regions, which did not seem to accord with Arago's view.

But gradually it was found that physicists had mistaken the character of the auroral display. It appeared that the magnetic needle not only swayed responsively to auroras observable in the immediate neighborhood, but to auroras in progress hundreds or even thousands of miles away. Nay, as inquiry progressed, it was discovered that the needles in our northern observatories are swayed by influences associated even with the occurrence of auroras around the southern polar regions.

In fact, not only have the difficulties pointed out (very properly; it need hardly be remarked) by Sir David Brewster been wholly removed; but it has been found that a much closer bond of sympathy exists between the magnetized needle and the auroral streamers than even Arago had supposed. It is not merely the case that while an auroral display is in progress the needle is subject to unusual disturbance, but the movements of the needle are actually synchronous with the waving movements of the mysterious streamers. An aurora may be in progress in the north of Europe, or even in Asia or America, and as the colored banners wave to and fro, the tiny needle, watched by patient observers at Greenwich or Paris, will respond to every phase of the display.

And I may notice in passing that two very interesting conclusions follow from this peculiarity: First, every magnetic needle over the whole earth must be simultaneously disturbed; and, secondly, the auroral streamers which wave across the skies of one country, must move synchronously with those which are visible in the skies of another country, even though thousands of miles may separate the two regions.

But I must pass on to consider further the circumstances which give interest and significance to the strange discovery which is the subject of this paper.

Could we only associate auroras with terrestrial magnetism, we should still have done much to enhance the interest which the beautiful phenomenon is calculated to excite. But when once this association has been established, others of even greater interest are brought into recognition. For terrestrial magnetism has been clearly shown to be influenced directly by the action of the sun. The needle in its daily vibration follows the sun, not indeed through a complete revolution, but as far as the influence of other forces will permit. This has been abundantly confirmed, and is a fact of extreme importance in the theory of terrestrial magnetism. Wherever the sun may be, either on the visible heavens or on that half of the celestial sphere which is at the moment beneath the horizon, the end of the needle nearest to the sun makes an effort (so to speak) to point more directly toward the great ruling centre of the planetary scheme. Seeing, then, that the daily vibration of the needle is thus caused, we recognize the fact that the disturbances of the daily vibration may be referred to some peculiarity of the solar action.

It was not, therefore, so surprising as many have supposed, that the increase and diminution of these disturbances, in a period of about eleven years, should be found to correspond with the increase and diminution of the number of solar spots in a period of equal length.

We already begin to see, then, that auroras are associated in some mysterious way with the action of the solar rays. The phenomenon which had been looked on for so many ages as a mere spectacle, caused perhaps by some process in the upper regions of the air, of a simply local character, has been brought into the range of planetary phenomena. As surely as the brilliant planets which deck the nocturnal skies are illuminated by the same orb which gives us our days and seasons, so are they subject to the same mysterious influence which causes the northern banners to wave resplendently over the starlit depths of heaven. Nay, it is even probable that every flicker and coruscation of our auroral displays corresponds with similar manifestations upon every planet which travels round the sun. It becomes, then, a question of exceeding interest to inquire what is the nature of the mysterious apparition which from time to time illuminates our skies. We have learned something of the laws according to which the aurora appears; but what is its true nature? What sort of light is that which illuminates the heavens? Is there some process of combustion going on in the upper regions of our atmosphere? Or are the auroral streamers electric or phosphorescent? Or, lastly, is the light simply solar light reflected from some substance which exists at an enormous elevation above the earth?

All these views have from time to time found supporters among scientific men. It need hardly be said that what we now know of the association between auroral action and some form of solar disturbance, would at once enable us to reject some of these hypotheses. But we need not discuss the subject from this point of view; because a mode of research has recently been rendered available which at once answers our inquiries as to the general character of any kind of light. I proceed to consider the application of this method to the light from the auroral streamers.

The spectroscope, or, as we may term the instrument, the "light-sifter," tells us of what nature an object which is a source of light may be. If the object is a luminous solid or liquid, the instrument converts its light into a rainbow-colored streak. If the object is a luminous vapor, its light is converted into a few bright lines. And, lastly, if the object is a luminous solid or liquid shining through any vapors, the rainbow-colored streak again makes its appearance, but it is now crossed by dark lines, corresponding to the vapors which surround the object and absorb a portion of its light.

But I must not omit to notice two circumstances which render the interpretation of a spectrum somewhat less simple than it would otherwise be.

In the first place, if an object is shining by reflected light, its spectrum is precisely similar to that of the object whose light illuminates it. Thus we cannot pronounce positively as to the nature of an object merely from the appearance of its spectrum, unless we are quite certain that the object is self-luminous. For example, we observe the solar spectrum to be a rainbow-colored streak crossed by a multitude of dark lines, and we conclude accordingly that the sun is an incandescent globe shining through a complex vaporous atmosphere. We feel no doubt on this point, because we are absolutely certain that the sun is self-luminous. Again, we observe the spectrum of the moon to be exactly similar to the solar spectrum, only, of course, much less brilliant. And here also we feel no doubt in interpreting the result. We know, certainly, that the moon is not self-luminous, and therefore we conclude with the utmost certainty that the light we receive from her is simply reflected solar light. So far all is clear. But now take the case of an object like a comet, which may or may not be self-luminous. If we find that a comet's spectrum resembles the sun's — and this is not altogether an hypothetical case, for a portion of the light of every comet yet examined does in reality give a rainbow-colored streak resembling the solar spectrum — we cannot form, in that case, any such positive conclusion. The comet may be a self-luminous body, but, on the other hand, its light may be due merely to the reflection of the solar beams. Accordingly, we find that our spectroscopists always accompany the record of such an observation with an expression of doubt as to the real nature of the object which is the source of light.

Secondly, when an electric spark flashes through any vapor, its light gives a spectrum which indicates the nature, not only of the vapor through which the spark has passed, but of the substances between which the spark has travelled. Thus, if we cause an electric flash to pass between iron points through common air, we see in the spectrum the numerous bright lines which form the spectrum of iron, and in addition we see the bright lines belonging to the gases which form our atmosphere.

Both the considerations above discussed are of the utmost importance in studying the subject of the auroral light as analyzed by the spectroscope, because there are many difficulties in forming a general opinion as to the nature of the auroral light, while there are circumstances which would lead us to anticipate that the light is electric.

We notice also in passing that we owe to the German physicist Angström a large share of the researches on which the above results respecting the spectrum of the electric spark are founded. The reader will presently see why we have brought Angström's name prominently forward in connection with the interesting branch of spectroscopic analysis just referred to. If the discovery we are approaching had been effected by a tyro in the use of the spectroscope, doubts might very reasonably have been entertained respecting the exactness of the observations on which the discovery rests.

It was suggested many years ago, long indeed before the true powers of spectroscopic analysis had been revealed, that perhaps if the light of the aurora were analyzed by the prism, evidence could be obtained of its electric nature. The eminent meteorologist Dové remarked, for instance, that "the peculiarities presented by the electric light are so marked that it appears easy to decide definitely by prismatic analysis, whether the light of the aurora is or is not electric." Singularly enough, however, the first proof that the auroral light is of an electric nature was derived from a very different mode of inquiry. Dr. Robinson, of Armagh, discovered in 1858 (a year before Kirchhoff's recognition of the powers of spectroscopic analysis) that the light of the aurora possesses in a peculiar degree a property termed fluorescence, which is a recognized and characteristic property of the light produced by electrical discharges. "These effects," he remarks of the appearances presented by the auroral light under the tests he applied, "were so strong in relation to the actual intensity of the light, that they appear to afford an additional evidence of the electric origin of the phenomenon."

Passing over this ingenious application of one of the most singular and interesting properties of light, we find that the earliest determination of the real nature of the auroral light — or rather of its spectrum — was that effected by Angström. This observer took advantage of the occurrence of a brilliant aurora in the winter of 1867-'68 to analyze the spectrum of the colored streamers. *A single bright line only was seen!* Otto Struve, an eminent Russian astronomer, shortly afterward made confirmatory observations. At the meeting of the Royal Astronomical Society in June, 1868, Mr. Huggins, F. R. S., thus described Struve's results: "In a letter, M. Otto Struve has informed me that he has had two good opportunities of observing the spectrum of the aurora borealis. The spectrum consists of one line, and the light is therefore monochromatic. The line falls near the margin of the yellow and green portions of the spectrum. . . . This shows that the monochromatic light is greenish, which surprised me; but General Sabine tells me that in his polar expeditions he has frequently seen the aurora tinged with green, and this appearance corresponds with the position of the line seen by M. Struve."

The general import of this observation there is no mistaking. It teaches us that the light of the aurora is due to luminous vapor, and we may conclude, with every appearance of probability, that the luminosity of the vapor is due to the passage of electric discharges through it. It is, however, possible that the position of the bright line may be due to the character of the particles between which the discharge takes place.

But the view we are to take must depend upon the position of the line. Here a difficulty presents itself. There is no known terrestrial element whose spectrum has a bright line precisely in the position of the line in the auroral spectrum. And mere proximity has no significance whatever in spectroscopic analysis. Two elements differing as much from each other in character as iron and hydrogen may have lines so closely approximating in position that only the most powerful spectroscope can indicate the difference. So that when Angström remarks that the bright line he has seen lies slightly to the left of a well-known group of lines belonging to the metal calcium (the principal ingredient of common chalk), we are by no means to infer that he supposes the substance which causes the presence of the bright line has any resemblance to that element. Until we can find an element which has a bright line in its spectrum absolutely coincident with the bright line detected by Angström in the spectrum of the aurora,* all speculation as to the real nature of the vapor in which the auroral electric discharge takes place, or of the substance between which the spark travels, is altogether precluded.

But interesting as the discovery undoubtedly is, we have now to deal with one of a yet more interesting character.

Most of my readers have doubtless heard of the zodiacal light, and many of them have perhaps seen that mysterious radiance, pointing obliquely upward from the western horizon soon after sunset in the spring months, or in autumn shortly before sunrise, above the eastern horizon. The light, as its name indeed implies, lies upon that region of the heavens along which the planets travel. Accordingly, astronomers have associated it with the planetary orbits, and have come to look on it as formed by the light reflected from a multitude of minute bodies travelling around the sun within the orbit of our earth.

Yet it had long been recognized, that there are difficulties in the way of this theory. Passing over those which depend on the position of the zodiacal light upon the heavens, there are difficulties connected with the appearance of the object. For example, its light has often been observed to flicker or coruscate in a manner which it seemed difficult to ascribe to the motions of our own atmosphere. Then again there have been seasons when the zodiacal light has shone with unusual intensity for months together, and there is nothing in the received theory which can account for such a peculiarity. Lastly, there is the strange circumstance recorded by Baron Humboldt that the zodiacal light is often invisible when night first sets in, and then suddenly appears with full splendor; a phenomenon which is utterly inexplicable if the received theory be accepted. The whole account of the phenomenon, as given by Baron Humboldt, is so interesting, and for my present purpose so significant, that I give it at full length:

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* Other green lines have since been discovered in the auroral spectrum; and occasionally a red line is seen.

"In the tropical climate of South America," he remarks, "the variable strength of the light of the zodiacal gleam struck me at times with utter amazement. As I there passed the beautiful nights, in the open air, on the banks of rivers, and in the grassy plains for several months together, I had opportunities of observing the phenomenon with attention. When the zodiacal light was at its very brightest, it sometimes happened that but a few minutes afterward it became notably weakened, and then it suddenly gleamed up again with its former brilliancy. In particular instances, I believed that I remarked — not any thing of a ruddy tinge, or an interior arched obscuration, or an emission of sparks, such as Mairan describes, but — a kind of unsteadiness and flickering of the light."

Despite these and similar observations, very little doubt had been felt by astronomers that the zodiacal light really indicates the presence of minute bodies travelling in more or less eccentric paths round the sun. And it was confidently expected that whenever a spectroscope of sufficient delicacy to analyze the faint light of the zodiacal gleam was applied to that purpose, the resulting spectrum would be merely a very faint reproduction of the solar spectrum.

Recently, however, the zodiacal light has been analyzed by Angström, with a result altogether unexpected, and at present almost unintelligible. *Its spectrum exhibits a bright line, and this bright line is the same that is seen in the spectrum of the aurora borealis!*

How are we to understand this most surprising result? Remembering that the aurora is undoubtedly a terrestrial light, whencesoever it derives its luminosity — in other words, that the electric discharges, however excited, really take place in the upper regions of our own atmosphere, while as certainly the zodiacal light is an extra-terrestrial phenomenon — the observed phenomenon becomes one of the most perplexing discoveries ever made by man. That it will before long be interpreted we have no doubt whatever; nor do we doubt that the interpretation will involve the explanation of a whole series of phenomena which have lately perplexed astronomers. Recalling the association between auroras and terrestrial magnetism, and that between terrestrial magnetism and the solar spots, and remembering further that our physicists have recently detected well-marked signs that the planets in their courses influence the sun's atmosphere and generate his spots in some manner as yet unexplained, we see that the one fact wanting to explain Angström's discovery is undoubtedly not an isolated fact, but must be associated in the most intimate manner with a variety of important cosmical relations. To speculate as to the nature of the as yet undiscovered interpretation of Angström's researches would at present be an idle task, perhaps. But one feature of the solar scheme with which we cannot doubt that it will be found to be associated, must be mentioned before we conclude.

Of all the phenomena presented to the contemplation of astronomers, the tails of comets are undoubtedly the most perplexing. Their rapid formation, their swift motions (if, indeed, we could believe that their changes of position are due to a real transmission of their material substance), and the enormous variety of configuration and of structure which they present to our contemplation, render them not merely amazing, but altogether unintelligible.

Now, there is one feature of comets' tails which has long since attracted attention, and will remind the reader of the peculiarities common to the zodiacal and the auroral light. We refer to the sudden changes of brilliancy, the flickerings or coruscations, and the instantaneous lengthening and shortening of these mysterious appendages. Olbers spoke of "explosions and pulsations which in a few seconds went trembling through the whole length of a comet's tail, with the effect now of lengthening, now of abridging it by several degrees." And the eminent mathematician Euler was led by the observation of similar appearances to put forward the theory "*that there is a great affinity between these tails, the zodiacal light, and the aurora borealis.*" The late Admiral Smyth, commenting on this opinion of Euler's, remarks that "most reasoners seem now to consider comets' tails as consisting of electric matter"; adding that "this would account for the undulations and other appearances which have been noticed, as, for instance, that extraordinary one seen by M. Chladni in the comet of 1811, when certain undulatory ebullitions rushed from the nucleus to the end of the tail, a distance of more than ten millions of miles, in two or three seconds of time." To this we may add the somewhat bizarre theory suggested by Sir John Herschel, that the matter forming the zodiacal light is "loaded, perhaps, with the actual materials of the tails of millions of comets, which have been stripped of these appendages in the course of successive passages round the immediate neighborhood of the sun."

Now, hitherto no comet with a sufficiently brilliant tail for spectroscopic analysis has appeared since Kirchhoff's invention of that mode of research. Already our physicists had been looking forward anxiously for the appearance of such a comet as Donati's or Halley's. But Angström's recent discovery, and the evidence which seems to associate the tails of comets with the auroral and zodiacal lights, render our spectroscopists doubly anxious to submit a comet's tail to spectroscopic analysis. It is far from being unlikely that three long-vexed questions — the nature of the aurora, and that of the zodiacal light, and that of comets' tails — will receive their solution simultaneously.

I had scarcely completed the above pages when news was brought from America that the spectrum of the sun's corona, as seen during the recent total solar eclipse, exhibited the same bright lines as the aurora. The fact that auroral *lines* are mentioned will at once be noticed; but it is to be remarked that the two faint lines which have been lately seen in the auroral spectrum correspond to but a very small portion of the light we receive from the northern streamers. In the spectrum of the corona the same three lines appear, but their relative brightness is different. The brightest line of the auroral spectrum is faint in the spectrum of the corona, while the latter exhibits a bright line where the former has a faint one.

News has also been received that a comparison of the photographs of the eclipse proves the corona, or at any rate its brightest part, to belong to the sun.

Lastly, it has been found that the peculiar phosphorescent light sometimes visible all over the sky at night gives the same spectrum (very faint, of course) as the aurora and the zodiacal light.

It is impossible not to recognize the fact that these discoveries point to relations of the utmost importance. The teachings of the spectroscope are too certain to be mistaken. When it shows us such and such lines bright or dark, we may conclude, without fear of being misled, that such and such substances are emitting or absorbing light. What we learn certainly, therefore, from the facts above stated, is this, that substances of the same sort emit the light of the aurora, of the zodiacal gleam, of the sun's corona, and of the phosphorescence which illuminates at times the nocturnal skies. We may conclude, but not so certainly, that the manner in which the light is emitted is also the same in each case. We know certainly that the auroral light is excited by the solar action. We know certainly that it is associated with the earth's magnetism. The opinion, then, which we should form of the source to which the other lights are due is tolerably obvious. So long as electricity was merely used as a convenient way of accounting for any perplexing phenomenon, it was impossible to accept explanations of cosmical peculiarities as due to electrical action. But when once we have reason — as in the case of the aurora we undoubtedly have — to associate electricity with any particular form of luminosity, we seem clearly justified in extending the explanation to the same form of luminosity wherever it may appear.

I believe that the key to the whole series of phenomena dealt with above lies in the existence of myriads of meteoric bodies travelling separately or in systems round the sun. They are consumed in thousands daily by our own atmosphere; they probably pour in countless millions upon the solar atmosphere; and from what we know of their numbers in our own neighborhood, and of the probability of their being infinitely more numerous in the neighborhood of the sun, we have excellent reasons for believing that to them principally is due the appearance of the zodiacal light and the solar corona.

THE EARTH A MAGNET.

There is a very prevalent but erroneous opinion that the magnetic needle points to the north. We remember well how we discovered in our boyhood that the needle does *not* point to the north, for the discovery was impressed upon us in a very unpleasant manner. We had purchased a pocket-compass, and were very anxious — not, indeed, to test the instrument, since we placed implicit reliance upon its indications — but to make use of it as a guide across unknown regions. Not many miles from where we lived lay Cobham Wood, no very extensive forest certainly, but large enough to lose one's self in. Thither, accordingly, we proceeded with three school-fellows. When we had lost ourselves, we gleefully called the compass into action, and made from the wood in a direction which we supposed would lead us home. We travelled on with full confidence in our pocket-guide; at each turning we consulted it in an artistic manner, carefully poising it and waiting till its vibrations ceased. But when we had travelled some two or three miles without seeing any house or road that we recognized, matters assumed a less cheerful aspect. We were unwilling to compromise our dignity as "explorers" by asking the way — a proceeding which no precedent in the history of our favorite travellers allowed us to think of. But evening came on, and with it a summer thunderstorm. We were getting thoroughly tired out, and the *hæc olim meminisse juvabit* with which we had been comforting ourselves began to lose its force. When at length we yielded, we learned that we had gone many miles out of our road, and we did not reach home till several hours after dark. How it fared with our school-fellows we know not, but a result overtook ourselves personally, for which there is no precedent, so far as we are aware, in the records of exploring expeditions. Also the offending compass was confiscated by justly indignant parents, so that for a long while the cause of our troubles was a mystery to us. We now know that instead of pointing due north, the compass pointed more than 20° toward the west, or nearly to the quarter called by sailors north-northwest. No wonder, therefore, that we went astray when we followed a guide so untrustworthy.

The peculiarity that the magnet needle does not, in general, point to the north, is the first of a series of peculiarities which we now propose briefly to describe. The irregularity is called by sailors the needle's *variation,* but the term more commonly used by scientific men is the *declination* of the needle. It was probably discovered a long time ago, for 800 years before our era the Chinese applied the magnet's directive force to guide them in journeying over the great Asiatic plains; and they must soon have detected so marked a peculiarity. Instead of a ship's compass, they made use of a magnetic car, on the front of which a floating needle carried a small figure, whose outstretched arm pointed southward. We have no record, however, of their discovery of the declination, and know only that they were acquainted with it in the twelfth century. The declination was discovered, independently, by European observers in the thirteenth century.

As we travel from place to place, the declination of the needle is found to vary. Christopher Columbus was the first to detect this. He discovered it on the 13th of September, 1492, during his first voyage, and when he was six hundred miles from Ferro, the most westerly of the Canary Islands. He found that the declination, which was toward the east in Europe, passed to the west, and increased continually as he travelled westward.

But here we see the first trace of a yet more singular peculiarity. We have said that at present the declination is toward the west in Europe. In Columbus's time it was toward the east. Thus we learn that the declination varies with the progress of time, as well as with change of place.

The genius of modern science is a weighing and a measuring one. Men are not satisfied nowadays with knowing that a peculiarity exists; they seek to determine its extent, how far it is variable — whether from time to time or from place to place, and so on. Now the results of such inquiries applied to the magnetic declination have proved exceedingly interesting.

We find, first, that the world may be divided into two unequal portions, over one of which the needle has a westerly, and over the other an easterly, declination. Along the boundary-line, of course, the needle points due north. England is situated in the region of westerly magnets. This region includes all Europe, except the northeastern parts of Russia; Turkey, Arabia, and the whole of Africa; the greater part of the Indian Ocean, and the western parts of Australia; nearly the whole of the Atlantic Ocean; Greenland, the eastern parts of Canada, and a small slice from the northeastern part of Brazil. All these form one region of westerly declination; but, singularly enough, there lies in the very heart of the remaining and larger region of easterly magnets an oval space of a contrary character. This space includes the Japanese Islands, Mantchooria, and the eastern parts of China. It is very noteworthy also, that in the westerly region the declination is much greater than in the easterly. Over the whole of Asia, for instance, the needle points almost due north. On the contrary, in the north of Greenland and of Baffin's Bay, the magnetic needle points due west; while still farther to the north (a little westerly), we find the needle pointing with its north end directly toward the south.

In the presence of these peculiarities, it would be pleasant to speculate. We might imagine the existence of powerfully magnetic *veins* in the earth's solid mass, coercing the magnetic needle from a full obedience to the true polar summons. Or the comparative effects of oceans and of continents might be called into play. But, unfortunately for all this, we have to reconcile views founded on *fixed* relations presented by the earth with the process of *change* indicated above. Let us consider the declination in England alone.

In the fifteenth century there was an easterly declination. This gradually diminished, so that in about the year 1657 the needle pointed due north. After this the needle pointed toward the west, and continually more and more, so that scientific men, having had experience only of a continual shifting of the needle in one direction, began to form the opinion that this change would continue, so that the needle would pass, through northwest and west, to the south. In fact, it was imagined that the motion of the needle would resemble that of the hands of a watch, only in a reversed direction. But before long observant men detected a gradual diminution in the needle's westerly motion. Arago, the distinguished French astronomer and physicist, was the first (we believe) to point out that "the progressive movement of the magnetic needle toward the west appeared to have become continually slower of late years" (he wrote in 1814), "which seemed to indicate that after some little time longer it might become retrograde." Three years later, namely, on the 10th of February, 1817, Arago asserted definitively that the retrograde movement of the magnetic needle had commenced to be perceptible. Colonel Beaufoy at first oppugned Arago's conclusion, for he found from observations made in London during the years 1817-1819, that the westerly motion still continued. But he had omitted to take notice of one very simple fact, viz., that London and Paris are two different places. A few years later the retrograde motion became perceptible at London also, and it has now been established by the observations of forty years. It appears from a careful comparison of Beaufoy's observations, that the needle reached the limit of its western digression (at Greenwich) in March, 1819, at which time the declination was very nearly 25°. In Paris, on the contrary, the needle had reached its greatest western digression (about 22½°) in 1814. It is rather singular that, although at Paris the retrograde motion thus presented itself five years earlier than in London, the needle pointed due north at Paris six years later than in London, viz., in 1663. Perhaps the greater amplitude of the needle's London digression may explain this peculiarity.

"It was already sufficiently difficult," says Arago, "to imagine what could be the kind of change in the constitution of the globe which could act during one hundred and fifty-three years in gradually transferring the direction of the magnetic needle from due north to 23° west of north. We see that it is now necessary to explain, moreover, how it has happened that this gradual change has ceased, and has given place to a return toward the preceding state of the globe." "How is it," he pertinently asks, "that the directive action of the globe, which clearly must result from the action of molecules of which the globe is composed, can be thus variable, while the number, position, and temperature of these molecules, and, as far as we know, all their other physical properties, remain constant?"

But we have considered only a single region of the earth's surface. Arago's opinion will seem still more just when we examine the change which has taken place in what we may term the "magnetic aspect" of the whole globe. The line which separates the region of westerly magnets from the region of easterly magnets now runs, as we have said, across Canada and eastern Brazil in one hemisphere, and across Russia, Asiatic Turkey, the Indian Ocean, and West Australia, in the other; besides having an outlying oval to the east of the Asiatic continent. Now these lines have swept round a part of the globe's circuit in a most singular manner since 1600. They have varied alike in direction and complexity. The Siberian oval, now distinct, was in 1787 merely a loop of the eastern line of no declination. The oval appears now to be continually diminishing, and will one day probably disappear.

We find here presented to us a phenomenon as mysterious, as astonishing, and as worthy of careful study, as any embraced in the wide domains of science. But other peculiarities await our notice.

If a magnetic needle of suitable length be carefully poised on a fine point, or, better, be suspended from a silk thread without torsion, it will be found to exhibit each day two small, but clearly perceptible, oscillations. M. Arago, from a careful series of observations, deduced the following results:

At about eleven at night, the north end of the needle begins to move from west to east, and having reached its greatest easterly excursion at about a quarter-past eight in the morning, returns toward the west to attain its greatest westerly excursion at a quarter-past one. It then moves again to the east, and having reached its greatest easterly excursion at half-past eight in the evening, returns to the west, and attains its greatest westerly excursion at eleven, as at starting.

Of course, these excursions take place on either side of the mean position of the needle, and as the excursions are small, never exceeding the fifth part of a degree, while the mean position of the needle lies some 20° to the west of north, it is clear that the excursions are only nominally eastern and western, the needle pointing, throughout, far to the west.

Now if we remember that the north end of the needle is that farthest from the sun, it will be easy to trace in M. Arago's results a sort of effort on the part of the needle to turn toward the sun — not merely when that luminary is above the horizon, but during his nocturnal path also.

We are prepared, therefore, to expect that a variation, having an annual period, shall appear, on a close observation of our suspended needle. Such a variation has been long since recognized. It is found that in the summer of both hemispheres, the daily variation is exaggerated, while in winter it is diminished.

But besides the divergence of a magnetized needle from the north pole, there is a divergence from the horizontal position which must now claim our attention. If a non-magnetic needle be carefully suspended so as to rest horizontally, and be then magnetized, it will be found no longer to preserve that position. The northern end *dips* very sensibly. This happens in our hemisphere. In the southern, it is the southern end which dips. It is clear, therefore, that if we travel from one hemisphere to the other, we must find the northern dip of the needle gradually diminishing, till at some point near the equator the needle is horizontal; and as we pass thence to southern regions, a gradually increasing southern inclination is presented. This has been found to be the case, and the position of the line along which there is no inclination (called the *magnetic equator*) has been traced around the globe. It is not coincident with the earth's equator, but crosses that circle at an angle of twelve degrees, passing from north to south of the equator in long. 3° west of Greenwich and from south to north in long. 187° east of Greenwich. The form of the line is not exactly that of a great circle, but presents here and there (and especially where it crosses the Atlantic) perceptible excursions from such a figure.

At two points on the earth's globe the needle will rest in a vertical position. These are the magnetic poles of the earth. The northern magnetic pole was reached by Sir J. G. Ross, and lies in 70° N. lat. and 263° E. long.; that is, to the north of the American Continent, and not very far from Boothia Gulf. One of the objects with which Ross set out on his celebrated expedition to the Antarctic Seas was the discovery, if possible, of the southern magnetic pole. In this he was not successful. Twice he was in hopes of attaining his object, but each time he was stopped by a barrier of land. He approached so near, however, to the pole, that the needle was inclined at an angle of nearly ninety degrees to the horizon, and he was able to assign to the southern pole a position in 75° S. lat., 154° E. long. It is not probable, we should imagine, that either pole is fixed, since we shall now see that the inclination, like the declination of the magnetic needle, is variable from time to time, as well as from place to place; and in particular, the magnetic equator is apparently subjected to a slow but uniform process of change.

Arago tells us that the inclination of the needle at Paris has been observed to diminish year by year since 1671. At that time the inclination was no less than 75°; in other words, the needle was inclined only 15° to the vertical. In 1791 the inclination was less than 71°. In 1831 it was less than 68°. In like manner, the inclination at London has been observed to diminish, from 72° in 1786 to 70° in 1804, and thence to 68° at the present time.

It might be anticipated from such changes as these that the magnetic equator would be found to be changing in position. Nay, we can even guess in which way it must be changing. For, since the inclination is diminishing at London and Paris, the magnetic equator must be approaching these places, and this (in the present position of the curve) can only happen by a gradual shifting of the magnetic equator from east to west along the true equator. This motion has been found to be really taking place. It is supposed that the movement is accompanied by a change of form; but more observations are necessary to establish this interesting point.

Can it be doubted that while these changes are taking place, the magnetic poles also are slowly shifting round the true pole? Must not the northern pole, for instance, be farther from Paris now that the needle is inclined more than 23° from the vertical, than in 1671, when the inclination was only 15°? It appears obvious that this must be so, and we deduce the interesting conclusion that each of the magnetic poles is rotating around the earth's axis.

But there is another peculiarity of the needle which is as noteworthy as any of those we have spoken about. We refer to the intensity of the magnetic action — the energy with which the needle seeks its position of rest. This is not only variable from place to place, but from time to time, and is further subject to sudden changes of a very singular character.

It might be expected that where the dip is greater, the directive energy of the magnet would be proportionately great. And this is found to be approximately the case. Accordingly, the magnetic equator is very nearly coincident with the "equator of least intensity," but not exactly. As we approach the magnetic poles we find a more considerable divergence, so that instead of there being a northern pole of greatest intensity nearly coincident with the northern magnetic pole, which we have seen lies to the north of the American Continent, there are *two* northern poles, one in Siberia nearly at the point where the river Lena crosses the Arctic circle, the other not so far to the north — only a few degrees north, in fact, of Lake Superior. In the south, in like manner, there are also two poles, one on the Antarctic circle, about 130° E. long., in Adelie Island, the other not yet precisely determined, but supposed to lie on about the 240th degree of longitude, and south of the Antarctic circle. Singularly enough, there is a line of lower intensity running right round the earth along the valleys of the two great oceans, "passing through Behring's Straits and bisecting the Pacific, on one side of the globe, and passing out of the Arctic Sea by Spitzbergen and down the Atlantic, on the other."

General Sabine discovered that the intensity of the magnetic action varies during the course of the year. It is greatest in December and January *in both hemispheres.* If the intensity had been greatest in winter, one would have been disposed to have assigned seasonal variation of temperature as the cause of the change. But as the epoch is the same for both hemispheres, we must seek another cause. Is there any astronomical element which seems to correspond with the law discovered by Sabine? There is one very important element. The position of the perihelion of the earth's orbit is such that the earth is nearest to the sun on about the 31st of December or the 1st of January. There seems nothing rashly speculative, then, in concluding that the sun exercises a magnetic influence on the earth, varying according to the distance of the earth from the sun. Nay, Sabine's results seem to point very distinctly to the law of variation. For, although the number of observations is not as yet very great, and the extreme delicacy of the variation renders the determination of its amount very difficult, enough has been done to show that in all probability the sun's influence varies according to the same law as gravity — that is, inversely as the square of the distance.

That the sun, the source of light and heat, and the great gravitating centre of the solar system, should exercise a magnetic influence upon the earth, and that this influence should vary according to the same law as gravity, or as the distribution of light and heat, will not appear perhaps very surprising. But the discovery by Sabine that *the moon* exercises a distinctly traceable effect upon the magnetic needle seems to us a very remarkable one. We receive very little light from the moon, much less (in comparison with the sun's light) than most persons would suppose, and we get absolutely no perceptible heat from her. Therefore it would seem rather to the influence of mass and proximity that the magnetic disturbances caused by the moon must be ascribed. But if the moon exercises an influence in this way, why should not the planets? We shall see that there is evidence of some such influence being exerted by these bodies.

More mysterious, if possible, than any of the facts we have discussed is the phenomenon of *magnetic storms*. The needle has been exhibiting for several weeks the most perfect uniformity of oscillation. Day after day, the careful microscopic observation of the needle's progress has revealed a steady swaying to and fro, such as may be seen in the masts of a stately ship at anchor on the scarce-heaving breast of ocean. Suddenly a change is noted; irregular jerking movements are perceptible, totally distinct from the regular periodic oscillations. A magnetic storm is in progress. But where is the centre of disturbance, and what are the limits of the storm? The answer is remarkable. If the jerking movements observed in places spread over very large regions of the earth — and in some well-authenticated cases over the whole earth — be compared with the local time, it is found that (allowance being made for difference of longitude) *they occur precisely at the same instant.* The magnetic vibrations thrill in one moment through the whole frame of our earth!

But a very singular circumstance is observed to characterize these magnetic storms. They are nearly always observed to be accompanied by the exhibition of the aurora in high latitudes, northern and southern. Probably they never happen without such a display; but numbers of auroras escape our notice. The converse proposition, however, *has* been established as a universal one. No great display of the aurora ever occurs without a strongly-marked magnetic storm.

Magnetic storms sometimes last for several hours or even days.

Remembering the influence which the sun has been found to exercise upon the magnetic needle, the question will naturally arise, Has the sun any thing to do with magnetic storms? We have clear evidence that he has.

On the 1st of September, 1859, Messrs. Carrington and Hodgson were observing the sun, one at Oxford and the other in London. Their scrutiny was directed to certain large spots which, at that time, marked the sun's face. Suddenly a bright light was seen by each observer to break out on the sun's surface and to travel, slowly in appearance, but in reality at the rate of about 7,000 miles in a minute, across a part of the solar disk. Now it was found afterward that the self-registering magnetic instruments at Kew had made at that very instant a strongly-marked jerk. It was learned that at that moment a magnetic storm prevailed at the West Indies, in South America, and in Australia. The signal-men in the telegraph-stations at Washington and Philadelphia received strong electric shocks; the pen of Bain's telegraph was followed by a flame of fire; and in Norway the telegraphic machinery was set on fire. At night great auroras were seen in both hemispheres. It is impossible not to connect these startling magnetic indications with the remarkable appearance observed upon the sun's disk.

But there is other evidence. Magnetic storms prevail more commonly in some years than in others. In those years in which they occur most frequently, it is found that the ordinary oscillations of the magnetic needle are more extensive than usual. Now, when these peculiarities had been noticed for many years, it was found that there was an alternate and systematic increase and diminution in the intensity of magnetic action, and that the period of the variation was about eleven years. But at the same time, a diligent observer had been recording the appearance of the sun's face from day to day and from year to year. He had found that the solar spots are in some years more freely displayed than in others. And he had determined the period in which the spots are successively presented with maximum frequency to be about eleven years. On a comparison of the two sets of observations, it was found (and has now been placed beyond a doubt by many years of continued observation) that magnetic perturbations are most energetic when the sun is most spotted, and *vice versa.*

For so remarkable a phenomenon as this none but a cosmical cause can suffice. We can neither say that the spots cause the magnetic storms, nor that the magnetic storms cause the spots. We must seek for a cause producing at once both sets of phenomena. There is as yet no certainty in this matter, but it seems as if philosophers would soon be able to trace in the disturbing action of the planets upon the solar atmosphere the cause as well of the marked period of eleven years as of other less distinctly-marked periods which a diligent observation of solar phenomena is beginning to educe.

OUR CHIEF TIMEPIECE LOSING TIME.

A DISTINGUISHED French astronomer, author of one of the most fascinating works on popular astronomy that has hitherto appeared, remarks that a man would be looked upon as a maniac who should speak of the influence of Jupiter's moons upon the cotton-trade. Yet, as he proceeds to show, there is an easily-traced connection between the ideas which appear at first sight so incongruous. The link is found in the determination of celestial longitude.

Similarly, what would be thought of an astronomer who, regarding thoughtfully the stately motion of the sidereal system, as exhibited on a magnified, and therefore appreciable, scale by a powerful telescope, should speak of the connection between this movement and the intrinsic worth of a sovereign? The natural thought with most men would be that "too much learning" had made the astronomer mad. Yet, when we come to inquire closely into the question of a sovereign's intrinsic value, we find ourselves led to the diurnal motion of the stars, and that by no very intricate path. For, What is a sovereign? A coin containing so many grains of gold mixed with so many grains of alloy. A grain, we know, is the weight of such and such a volume of a certain standard substance — that is, so many cubic inches, or parts of a cubic inch, of that substance. But what is an inch? It is determined, we find, as a certain fraction of the length of a pendulum vibrating seconds in the latitude of London. A second, we know, is a certain portion of a mean solar day, and is practically determined by a reference to what is called a sidereal day — the interval, namely, between the successive passages by the same star of the celestial meridian of any fixed place. This interval is assumed to be constant, and it has indeed been described as the "one constant element" known to astronomers.

We find, then, that there is a connection, and a very important connection, between the motion of the stars and our measures, not merely of value, but of weight, length, volume, and time. In fact, our whole system of weights and measures is founded on the apparent diurnal motion of the sidereal system, that is, on the real diurnal rotation of the earth. We may look on the meridian-plane in which the great transit-telescope of the Greenwich Observatory is made to swing, as the gigantic hand of a mighty dial, a hand which, extending outward among the stars, traces out for us, by its motion among them, the exact progress of time, and so gives us the means of weighing, measuring, and valuing terrestrial objects with an exactitude which is at present *beyond* our wants.

The earth, then, is our "chief timepiece," and it is of the correctness of this giant clock that we are now to speak.

But how can we test a timepiece whose motions we select to regulate every other timepiece? If a man sets his watch every morning by the clock at Westminster, it is clearly impossible for him to test the accuracy of that clock by the motions of his watch. It would, indeed, be possible to detect any gross change of rate; but, for the purpose of illustration, I assume, what is indeed the case, that the clock is very accurate, and, therefore, that minute errors only are to be looked for even in long intervals of time. And, just as the watch set by a clock cannot be made use of to test the clock for small errors, so our best timepieces cannot be employed to detect slow variations, if any such exist, in the earth's rotation-period.

Sir William Herschel, who early saw the importance of the subject, suggested another method. Some of the planets rotate in such a manner, and bear such distinct marks upon their surfact, that it is possible, by a series of observations extending over a long interval of time, to determine the length of their rotation-period within a second or two. Supposing their rotation uniform, we at once obtain an accurate measure of time. Supposing their rotation *not* uniform, we obtain — (1) a hint of the kind of change we are looking for; and (2), by the comparison of two or more planets, the means of guessing how the variation is to be distributed between the observed planets and our own earth.

Unfortunately, it turned out that Jupiter, one of the planets from which Herschel expected most, does *not* afford us exact information — his real surface being always veiled by his dense and vapor-laden atmosphere. Saturn, Venus, and Mercury, are similarly circumstanced, and are in other respects unfavorable objects for this sort of observation. Mars only, of all the planets, is really available. Distinctly marked (in telescopes of sufficient power) with continents and oceans, which are rarely concealed by vapors, this planet is in other respects fortunately situated. For it is certain that whatever variations may be taking place in planetary rotations must be due to external agencies. Now, Saturn and Jupiter have their satellites to influence (perhaps appreciably in long intervals of time) their rotation-movements. Venus and Mercury are near the sun, and are therefore in this respect worse off than the earth, whose rotation is in question. Mars, on the other hand, farther removed than we are from the sun, having also no moon, and being of small dimensions (a very important point, be it observed, since the tidal action of the sun depends on the dimensions of a planet), is likely to have a rotation-period all but absolutely constant.

Herschel was rather unfortunate in his observations of Mars. Having obtained a rough approximation from Mars' rotation in an interval of two days — this rough approximation being, as it happened, only thirty-seven seconds in excess of the true period — he proceeded to take three intervals of one month each. This should have given a much better value, but, as it happened, the mean of the values he obtained was forty-six seconds too great. He then took a period of two years, and being misled by the erroneous values he had already obtained, he *missed one rotation,* getting a value two minutes too great. Thirty years ago, two German astronomers, Beer and Mädler, tried the same problem, and taking a period of seven years, obtained a value which exceeds the true value by only one second. Another German, Kaiser, by combining more observations, obtained a value which is within one-fifteenth of a second of the true value. But a comparison of observations extending over 200 years has enabled the present writer to obtain a value which he considers to lie one-hundredth part of a second of the truth. This value for Mars' rotation-period is 24 hours 37 minutes 22.74 seconds.

Here, then, we have a result so accurate, that *at some future time,* it may serve to test the earth's rotation-period. We have compared the rotation-rate of our test-planet with the earth's rate during the past 200 years; and therefore, if the earth's rate vary by more than one-hundredth of a second in the next two or three hundred years, we shall — or, rather, our descendants will — begin to have some notion of the change at the end of that time.

But, in the mean time, mankind being impatient, and not willing to leave to a distant posterity any question which can possibly be answered *now,* astronomers have looked around them for information available at once on this interesting point. The search has not been in vain. In fact, we are able to announce, with an approach to positiveness, that our great terrestrial timepiece is actually *losing time.*

In our moon we have a neighbor which has long been in the habit of answering truthfully questions addressed to her by astronomers. Of old, she told Newton about gravitation, and when he doubted, and urged contradictory evidence offered — as men in his time supposed — by the earth, she set him on the right track, so that when in due time the evidence offered by the earth was corrected, Newton was prepared at once to accept and propound the noble theory which rendered his name illustrious. Again, men wished to learn the true shape of the earth, and went hither and thither measuring its globe; but the moon, meanwhile, told the astronomer who remained at home a truer tale. They sought to learn the earth's distance from the sun, and from this and that point they turned their telescopes on Venus in transit; but the moon has set them nearer the truth, and that not by a few miles, but by 3,000,000 or more. We shall see that she has had something to say about our great terrestrial timepiece.

One of the great charms of the science of astronomy is, that it enables men to *predict.* At such and such an hour, the astronomer is able to say, a celestial body will occupy such and such a point on the celestial sphere. You direct a telescope toward the point named, and lo! at the given instant the promised orb sweeps across the field of view. Each year there is issued a thick octavo volume crowded with such predictions, three or four years in advance of the events predicted; and these predictions are accepted with as little doubt by astronomers as if they were the records of past events.

But astronomers are not only able to predict — they can also trace back the paths of the celestial bodies, and say: "At such and such a long-past epoch, a given star or planet occupied such and such a position upon the celestial sphere." But how are they to verify such a statement? It is clear that, in general, they cannot do so. Those who are able to appreciate (or better, to make use of) the predictions of astronomy, will, indeed, very readily accord a full measure of confidence to calculations of past events. They know that astronomy is justly named the most exact of the sciences, and they can see that there is nothing, in the nature of things, to render retrospection more difficult than prevision. But there are hundreds who have no such experience of the exactness of modern astronomical methods — who have, on the contrary, a vague notion that modern astronomy is merely the successor of systems now exploded; perhaps even that it may one day have to make way in its turn for new methods. And if all other men were willing to accept the calculations of astronomers respecting long-past events, astronomers themselves would be less easily satisfied. Long experience has taught them that the detection of error is the most fruitful source of knowledge; therefore, wherever such a course is possible, they always gladly submit their calculations to the test of observation.

Now, looking backward into the far past, it is only here and there that we see records which afford means of comparison with modern calculations. The planets have swept on in their courses for ages with none to note them. Gradually, observant men began to notice and record the more remarkable phenomena. But such records, made with very insufficient instrumental means, have in general but little actual value. It has been found easy to confirm them without any special regard to accuracy of calculation.

But there is one class of phenomena which no inaccuracy of observation can very greatly affect. A total eclipse of the sun is an occurrence so remarkable, that (1) it can hardly take place without being recorded, and (2) a very rough record will suffice to determine the particular eclipse referred to. Long intervals elapse between successive total eclipses visible at the same place on the earth's surface; and even partial eclipses of noteworthy extent occur but seldom at any assigned place. Very early, therefore, in the history of modern astronomy, the suggestion was made, that eclipses recorded by ancient historians should be calculated retrospectively. An unexpected result rewarded the undertaking: it was found that ancient eclipses could not be fairly accounted for without assigning a slower motion to the moon in long-past ages than she has at present!

Here was a difficulty which long puzzled mathematicians. One after another was foiled by it. Halley, an English mathematician, had detected the difficulty, but no English mathematician was able to grapple with it. Contented with Newton's fame, they had suffered their Continental rivals to shoot far ahead in the course he had pointed out. But the best Continental mathematicians were defeated. In papers of acknowledged merit, adorned by a variety of new processes, and showing a deep insight into the question at issue, they yet arrived, one and all, at the same conclusion — failure.

Ninety years elapsed before the true explanation was offered by the great mathematician Laplace. A full exposition of his views would be out of place in such a paper as the present, but briefly, they amount to this:

The moon travels in her orbit, swayed chiefly by the earth's attraction. But the sun, though greatly more distant, yet, owing to the immensity of his mass, plays an important part in guiding our satellite. His influence tends to relieve the moon, in part, from the earth's sway. Thus she travels in a wider orbit, and with a slower motion, than she would have but for the sun's influence. Now, the earth is not at all times equally distant from the sun, and his influence upon the moon is accordingly variable. In winter, when the earth is nearest to the sun, his influence is greatest. The lunar month, accordingly (as any one may see by referring to an almanac), is longer in winter than in summer. This variation had long been recognized as the moon's "annual equation"; but Laplace was the first to point out that the variation is itself slowly varying. The earth's orbit is slowly changing in shape — becoming more and more nearly circular year by year. As the greater axis of her orbit is unchanging, it is clear that the actual extent of the orbit is slowly increasing. Thus, the moon is slightly released from the sun's influence year by year, and so brought more and more under the earth's influence. She travels, therefore, continually faster and faster; though the change is indeed but a very minute one — only to be detected in long intervals of time. Also the moon's *acceleration,* as the change is termed, is only temporary, and will in due time be replaced by an equally gradual retardation.

When Laplace had calculated the extent of the change due to the cause he had detected, and when it was found that ancient eclipses were now satisfactorily accounted for, it may well be believed that there was triumph in the mathematical camp. But this was not all. Other mathematicians attacked the same problem, and their results agreed so closely that all were convinced that the difficulty was thoroughly vanquished.

A very noteworthy result flowed from Laplace's calculations. Among other solutions which had been suggested, was the supposition (supported by no less an authority than Sir Isaac Newton, who lived to see the commencement of the long conflict maintained by mathematicians with this difficulty), that it is not the moon travelling more quickly, but our earth rotating more slowly, which causes the observed discrepancy. Now, it resulted from Laplace's labors — as he was the first to announce — that the period of the earth's rotation has not varied by one-tenth of a second per century in the last two thousand years. The question thus satisfactorily settled, as was supposed, was shelved for more than a quarter of a century. The result, also, which seemed to flow from the discussion — the constancy of the earth's rotation-movement — was accepted; and, as we have seen, our national system of measures was founded upon the assumed constancy of the day's duration.

But mathematicians were premature in their rejoicings. The question has been brought, by the labors of Professor Adams — co-discoverer with Leverrier of the distant Neptune — almost exactly to the point which it occupied a century ago. We are face to face with the very difficulties — somewhat modified in extent but not in character — which puzzled Halley, Euler, and Lagrange. It would be an injustice to the memory of Laplace to say that his labors were thrown away. The explanation offered by him is indeed a just one, but it is insufficient. Properly estimated, it removes only half the difficulty which had perplexed mathematicians. It would be quite impossible to present in brief space, and in form suited to these pages, the views propounded by Adams. What, for instance, would most of our readers learn if we were to tell them that, "when the variability of the eccentricity is taken into account, in integrating the differential equations involved in the problem of the lunar motions — that is, when the eccentricity is made a function of the time — non-periodic or secular terms appear in the expression for the moon's mean motion"— and so on? Let it suffice to say that

Laplace had considered only the effect of the sun in diminishing the earth's *pull* on the moon, supposing that the slow variation in the sun's *direct* influence on the moon's motion in her orbit must be self-compensatory in long intervals of time. Adams has shown, on the contrary, that when this variation is closely examined, no such compensation is found to take place; and that the effect of this want of compensation is to diminish by more than one-half the effects due to the slow variation examined by Laplace.

These views gave rise at first to considerable controversy. Pontecoulant characterized Adams's processes as "analytical conjuring-tricks"; and Leverrier stood up gallantly in defence of Laplace. The contest swayed hither and thither for a while; but gradually the press of new arrivals on Adams's side began to prevail. One by one, his antagonists gave way; new processes have confirmed his results, figure for figure; and no doubt now exists, in the mind of any astronomer competent to judge, of the correctness of Adams's views.

But side by side with this inquiry, another had been in progress. A crowd of diligent laborers had been searching with close and rigid scrutiny into the circumstances attending ancient eclipses. A new light had been thrown upon this subject by the labors of modern travellers and historians. One remarkable instance of this may be cited. Mr. Layard has identified the site of Larissa with the modern Nimroud. Now, Xenophon relates that when Larissa was besieged by the Persians, an eclipse of the sun took place, so remarkable in its effects (and therefore undoubtedly total), that the Median defenders of the town threw down their arms and the city was accordingly captured. And Hansen had shown that a certain estimate of the moon's motion makes the eclipse which occurred on August 15, 310 B. C., not only total, but central at Nimroud. Some other remarkable eclipses — as the celebrated sunset eclipse (total) at Rome 399 B. C., the eclipse which enveloped the fleet of Agathocles as he escaped from Syracuse; the famous eclipse of Thales, which interrupted a battle between the Medes and Lydians; and even the partial eclipse which (probably) caused the "going back of the shadow upon the dial of Ahaz"— have all been accounted for satisfactorily by Hansen's estimate of the moon's motion; so, also, have nineteen lunar eclipses recorded in the Almagest.

This estimate of Hansen's which accounts so satisfactorily for solar and lunar eclipses, makes the moon's rate of motion increase more than twice as fast as it should do according to the calculations of Adams. But before our readers run away with the notion that astronomers have here gone quite astray, it will be well to present, in a simple manner, the extreme minuteness of the discrepancy about which all the coil has been made.

Suppose that, just in front of our moon, a false moon exactly equal to ours in size and appearance (see *note* at the end of this paper) were to set off with a motion corresponding to the present motion of the moon, save only in one respect — namely, that the false moon's motion should not be subject to the change we are considering, termed *the acceleration.* Then one hundred years would elapse before our moon would fairly begin to show in advance. She would, in that time, have brought only one-one-hundred-and-fiftieth part of her breadth from behind the false moon. At the end of another century, she would have gained four times as much; at the end of a third, nine times as much: and so on. She would not fairly have cleared her own breadth in less than twelve hundred years. But the *whole* of this gain, minute as it is, is not left unaccounted for by our modern astronomical theories. *Half* the gain is explained, the other half remains to be interpreted; in other words, *the moon travels farther by about half her own breadth in twelve centuries than she should do according to the lunar theory.*

But in this difficulty, small as it seems, we are not left wholly without resource. We are not only able to say that the discrepancy is probably due to a gradual retardation of the earth's rotation-movement, but we are able to place our finger on a very sufficient cause for such a retardation. One of the most firmly-established principles of modern science is this — that where *work is done,* force is, in some way or other, expended. The *doing of work* may show itself in a variety of ways — in the generation of heat, in the production of light, in the raising of weights, and so on; but in every case an equivalent force must be expended. If the brakes are applied to a train in motion, intense heat is generated in the substance of the brake; now, the force employed by the brakesman is *not* equivalent to the heat generated. Where, then, is the balance of force expended? We all know that the train's motion is retarded, and this loss of motion represents the requisite expenditure of force. Now, is there any process in Nature resembling, in however remote a degree, the application of a brake to check the earth's rotation? There is. The tidal wave, which sweeps, twice a day, round the earth, travels in a direction contrary to the earth's motion of rotation. That this wave "does work," no one can doubt who has watched its effects. The mere rise and fall in open ocean may not be strikingly indicative of "work done"; but when we see the behavior of the tidal wave in narrow channels, when we see heavily-laden ships swept steadily up our tidal rivers, we cannot but recognize the expenditure of force. Now, where does this force come from? Motion being the great "force-measurer," what motion *suffers* that the tides may *work?* We may securely reply, that the only motion which *can* supply the requisite force is the earth's motion of rotation. Therefore, it is no idle dream, but a matter of absolute certainty, that, though slowly, still very surely, our terrestrial globe is losing its rotation-movement.

Considered as a timepiece, what are the earth's errors? Suppose, for a moment, that the earth was *timed* and *rated* two thousand years ago, how much has she *lost,* and what is her "rate error"? She has lost in that interval nearly one hour and a quarter, and she is losing now at the rate of one second in twelve weeks. In other words, the length of a day is now more by about one-eighty-fourth part of a second than it was two thousand years ago. At this rate of change, our day would emerge into a lunar month in the course of thirty-six thousand millions of years. But after a while, the change will take place more slowly, and some trillion or so of years will elapse before the full change is effected.

Distant, however, as is the epoch at which the changes we have been considering will become effective, the subject appears to us to have an interest apart from the mere speculative consideration of the future physical condition of our globe. Instead of the recurrence of ever-varying, closely-intermingled cycles of fluctuation, we see, now for the first time, the evidence of cosmical decay — a decay which in its slow progress, may be but the preparation for renewed genesis — but still, a decay which, so far as the races at present subsisting upon the earth are concerned, must be looked upon as finally and completely destructive.

ENCKE THE ASTRONOMER.

Four years have passed since Encke died. Even those four years have witnessed notable changes in the aspect of the science he loved so well. But we must look back over more than fifty years, if we would form an estimate of the position of astronomy when Encke's most notable work was achieved. At Seeberge, under Lindenau, Encke had been perfecting himself in the higher branches of mathematical calculation. He took the difficult work of determining the orbital motions of newly-discovered comets under his special charge, and Dr. Bruhns tells us that every comet which was detected during Encke's stay at Seeberge was subjected to rigid scrutiny by the indefatigable mathematician. Before long a discovery of the utmost importance rewarded his persevering labors. Pons had detected on November 26, 1818, a comet of no very brilliant aspect, which was watched first at Marseilles, and then at Mannheim, until December 29th. Encke next took up the work, and tracked the comet until January 12th. Combining the observations made between December 22d and January 12th, he assigned to the body a parabolic orbit. But he was not satisfied with the accordance between this path and the observed motions of the body. When he attempted to account for the motions of the comet by means of an orbit of comparatively short period, he was struck by the resemblance between the path thus deduced and that of Comet I, 1805. Gradually the idea dawned upon him that a new era was opening for science. Hitherto the only periodical comets which had been discovered except Lexell's — the "lost comet"— had travelled in orbits extending far out into space beyond the paths of the most distant known planets. But now Encke saw reason to believe that he had to deal with a comet travelling within the orbit of Jupiter. On February 5th, he wrote to the eminent mathematician Gauss, pointing out the results of his inquiries, and saying that he only waited for the encouragement and authority of his former teacher to prosecute his researches to the end toward which they already seemed to point. Gauss, in reply, not only encouraged Encke to proceed, but counselled him as to the course he should pursue. The result we all know. Encke showed conclusively that the newly-discovered comet travels in a path of short period, and that it had already made its appearance several times in our neighborhood.

From the date of this discovery, Encke took high rank among the astronomers of Europe. His subsequent labors by no means fell short of the promise which this, his first notable achievement, had afforded. If he effected less as an astronomical observer than many of his contemporaries, he was surpassed by few as a manipulator of those abstruse formulæ by which the planetary perturbations are calculated. It was to the confidence engendered by this skill that we owe his celebrated discovery of the acceleration of the motion of the comet mentioned above. Assured that he had rightly estimated the disturbances to which the comet is subjected, he was able to pronounce confidently that some cause continually (though all but imperceptibly) impedes the passage of this body through space, and so — by one of those strange relations which the student of astronomy is familiar with — the continually retarded comet travels ever more swiftly along a continually diminishing orbit.

Bruhns's Life of Encke is well worth reading, not only by those who are interested in Encke's fame and work as an astronomer, but by the general reader. Encke the man is presented to our view, as well as Encke the astronomer. With loving pains the pupil of the great astronomer handles the theme he has selected. The boyhood of Encke, his studies, his soldier-life in the great uprising against Napoleon in 1813, and his work at the Seeberge Observatory; his labors on comets and asteroids; his investigations of the transits of 1761 and 1769; his life as an academician, and as director of an important observatory; his orations at festival and funeral; and lastly, his illness and death, are described in these pages by one who held Encke in grateful remembrance as "teacher and master," and as "a fatherly friend."

Not the least interesting feature of the work is the correspondence introduced into its pages. We find Encke in communication with Humboldt, with Bessel and Struve, with Hansen, Olbers, and Argelander; with a host, in fine, of living as well as of departed men of science.

VENUS ON THE SUN'S FACE.

MORE than a century ago scientific men were looking forward with eager interest to the passage of the planet Venus across the sun's face in 1769. The Royal Society judged the approaching event to be of such extreme importance to the science of astronomy, that they presented a memorial to King George III., requesting that a vessel might be fitted out, at government expense, to convey skilful observers to one of the stations which had been judged suitable for observing the phenomenon. The petition was complied with, and, after some difficulty as to the choice of a leader, the good ship "Endeavor," of 370 tons, was placed under the command of Captain Cook. The astronomical work intrusted to the expedition was completely successful; and thus it was held that England had satisfactorily discharged her part of the work of utilizing the rare phenomenon known as a transit of Venus.

A century passes, and science is again awaiting with interest the approach of one of these transits. But now her demands are enlarged. It is not one ship that is asked for, but the full cost and charge of several expeditions. And this time, also, science has been more careful in taking time by the forelock. The first hints of her requirements were heard some fourteen years ago, when the Astronomer-Royal began that process of laborious inquiry which a question of this sort necessarily demands. Gradually, her hints became more and more plain-spoken; insomuch that Mr. Airy — her mouth-piece in this case — stated definitely, a few months ago, what he thought science had a right to claim from England in this matter. When the claim came before our government, it was met with a liberality which was a pleasing surprise after Mr. Lowe's placid reference of scientific people to their own devices. The sum of ten thousand five hundred pounds has been granted to meet the cost of several important and well-appointed expeditions; and doubtless further material aid will be derived from the various government observatories.

And now let us inquire why so much interest is attached to a phenomenon which appears, at first sight, to be so insignificant. Transits, eclipses, and other phenomena of that nature, are continually occurring, without any particular interest being attached to them. The telescopist may see half a dozen such phenomena in the course of a night or two, by simply watching the satellites of Jupiter, or the passage of our moon over the stars. Even the great eclipse of 1868 did not attract so much interest as the coming transit of Venus, yet that eclipse had never been equalled in importance by any which has occurred in historic times, and hundreds of years must pass before such another happens, whereas transits of Venus are far from being so uncommon.

The fact is, that Venus gives us the best means we have of mastering a problem which is one of the most important within the whole range of the science of astronomy. We use the term important, of course, with reference to the scientific significance and interest of the problem. Practically, it matters little to us whether the sun is a million of miles or a thousand millions of miles from us. The subject must in any case be looked upon as an extra-parochial one. But science does occasionally attach immense interest to extra-parochial subjects. And this is neither unwise nor unreasonable, since we find implanted in our very nature — and not merely in the nature of scientific men — a quality which causes us to take interest in a variety of matters that do not in the least concern our personal interests. Nor is this quality, rightly considered, one of the least noble characteristics of the human race.

That the determination of the sun's distance is important, in an astronomical sense, will be seen at once when it is remembered that the ideas we form of the dimensions of the solar system are wholly dependent on our estimate of the sun's distance. Nor can we gauge the celestial depths with any feeling of assurance, unless we know the true length of that which is our sole measuring-rod. It is, in fact, our basis of measurement for the whole visible universe. In some respects, even if we knew the sun's distance exactly, it would still be an unsatisfactory gauge for the stellar depths. But that is the misfortune, not the fault of the astronomer, who must be content to use the measuring-rod which Nature gives him. All he can do is to find out as nearly as he can its true length.

When we come to consider how the astronomer is to determine this very element — the sun's distance — we find that he is hampered with a difficulty of precisely the same character.

The sun being an inaccessible object, the astronomer can apply no other methods to determine its distance — directly — than those which a surveyor would use in determining the distance of an inaccessible castle, or rock, or tree, or the like. We shall see presently that the ingenuity of astronomers has, in fact, suggested some other indirect methods. But clearly the most satisfactory estimate we can have of the sun's distance is one founded on such simple notions and involving in the main such processes of calculation as we have to deal with in ordinary surveying.

There is, in this respect, no mystery about the solution of the famous problem. Unfortunately, there is enormous difficulty.

When a surveyor has to determine the distance of an inaccessible object, he proceeds in the following manner: He first very carefully measures a base-line of convenient length. Then from either end of the base-line he takes the bearings of the inaccessible object — that is, he observes the direction in which it lies. It is clear that, if he were now to draw a figure on paper, laying down the base-line to some convenient scale, and drawing lines from its ends in directions corresponding to the bearings of the observed object, these lines would indicate, by their intersection, the true relative position of the object. In practice, the mathematician does not trust to so rough a method as construction, but applies processes of calculation.

Now, it is clear that in this plan every thing depends on the base-line. It must not be too short in comparison with the distance of the inaccessible object; for then, if we make the least error in observing the bearings of the object, we get an important error in the resulting determination of the distances. The reader can easily convince himself of this by drawing an illustrative case or two on paper.

The astronomer has to take his base-line for determining the sun's distance, upon our earth, which is quite a tiny speck in comparison with the vast distance which separates us from the sun. It had been found difficult enough to determine the moon's distance with such a short base-line to work from. But the moon is only about a quarter of a million of miles from us, while the sun is more than ninety millions of miles off. Thus the problem was made several hundred times more difficult — or, to speak more correctly, it was rendered simply insoluble unless the astronomer could devise some mode of observing which should vastly enhance the power of his instruments.

For, let us consider an illustrative case. Suppose there were a steeple five miles off, and we had a base-line only two feet long. That would correspond as nearly as possible to the case the astronomer has to deal with. Now, what change of direction could be observed in the steeple by merely shifting the eye along a line of two feet? There is a ready way of answering. Invert the matter. Consider what a line of two feet long would look like if viewed from a distance of five miles. Would its length be appreciable, to say nothing of its being measurable? Yet it is just such a problem as the measurement of that line which the astronomer would have to solve.

But even this is not all. In our illustration only one observer is concerned, and he would be able to use one set of instruments. Suppose, however, that from one end of the two-feet line an observer using one set of instruments took the bearing of the steeple; and that, half a year after, another observer brought another set of instruments and took the bearing of the steeple from the other end of the two-feet line, is it not obvious how enormously the uncertainty of the result would be increased by such an arrangement as this? One observer would have his own peculiar powers of observation, his own peculiar weaknesses; the other would have different peculiarities. One set of instruments would be characterized by its own faults or merits, so would the other. One series of observations would be made in summer, with all the disturbing effects due to heat; the other would be made in winter, with all the disturbing effects due to cold.

The observation of the sun is characterized by all these difficulties. Limited to the base-lines he can measure on earth, the astronomer must set one observer in one hemisphere, another in the other. Each observer must have his own set of instruments; and every observation which one has made in summer will have to be compared with an observation which the other has made in winter.

Thus we can understand that astronomers should have failed totally when they attempted to determine the sun's distance without aid from the other celestial bodies.

It may seem at first sight as though nothing the other celestial bodies could tell the astronomer would be of the least use to him, since these bodies are for the most part farther off than the sun, and even those which approach nearest to us are still far beyond the limits of distance within which the simple plan followed by surveyors could be of any service. And besides, it might be supposed that information about the distance of one celestial body could be of no particular service toward the determination of the distance of another.

But two things aid the astronomer at this point: First of all, he has discovered the law which associates together the distances of all the planets from the sun; so that if he can determine the distance of any one planet he learns immediately the distances of all. Secondly, the planets in their motion travel occasionally into such positions that they become mighty indices, tracing out on a natural dial-plate the significant lesson from which the astronomer hopes to learn so much. To take an instance from the motions of another planet than the one we are dealing with. Mars comes sometimes so near the earth that the distance separating us from him is little more than one-third of that which separates us from the sun. Suppose that, at such a time, he is seen quite close to a fixed star. That star gives the astronomer powerful aid in determining the planet's distance. For, to observers in some parts of the earth, the planet will seem nearer to the star than he will to observers elsewhere. A careful comparison of the effects thus exhibited will give significant evidence respecting the distance of Mars. And we see that the star has served as a fixed mark upon the vast natural dial of the heavens, just as the division-marks on a clock-face serve to indicate the position of the hands.

Now, we can at once see why Venus holds so important a position in this sort of inquiry. Venus is our nearest neighbor among the planets. She comes several millions of miles nearer to us than Mars, our next neighbor on the other side. That is the primary reason of her being so much considered by astronomers. But there is another of equal importance. Venus travels nearer than our earth to the sun. And thus there are occasions when she gets directly between the earth and the sun. At those times she is seen upon his face, and his face serves as a dial-plate by which to measure her movements. When an observer at one part of the earth sees her on one part of the sun's face, another observer at some other part of the earth will see her on another, and the difference of position, if accurately measured, would at once indicate the sun's distance. As a matter of fact, other modes of reading off the indications of the great dial-plate have to be adopted. Before proceeding to consider those modes, however, we must deal with one or two facts about Venus's movements which largely affect the question at issue.

Let us first see what we gain by considering the distance of Venus rather than that of the sun.

At the time of a transit Venus is of course on a line between the earth and the sun, and she is at somewhat less than a third of the sun's distance from us. Thus whatever effect an observer's change of place would produce upon the sun would be more than trebled in the case of Venus. But it must not be forgotten that we are to judge the motions of Venus by means of the dial-plate formed by the solar disk, and that dial-plate is itself shifted as the observer shifts his place. Venus is shifted three times as much, it is true; but it is only the balance of change that our astronomer can recognize. That balance is, of course, rather more than twice as great as the sun's change of place.

So far, then, we have not gained much, since it has been already mentioned that the sun's change of place is not measurable by any process of observation astronomers can apply.

It is to the fact that we have the sun's disk whereby to measure the change that we must chiefly trust; and even that would be insufficient were it not for the fact that Venus is not at rest, but travels athwart the great solar dial-plate. We are thus enabled to make a time measurement take the place of a measurement of space. If an observer in one place sees Venus cross the sun's face at a certain distance from the centre, while an observer at another place sees her follow a path slightly farther from the centre, the transit will clearly seem longer to the former observer than to the latter.

This artifice of exchanging a measurement of time for one of space — or *vice versa* — is a very common one among astronomers. It was Edmund Halley, the friend and pupil of Sir Isaac Newton, who suggested its application in the way above described. It will be noticed that what is required for the successful application of the method is that one set of observers should be as far to the north as possible, another as far to the south, so that the path of Venus may be shifted as much as possible. Clearly the northern observers will see her path shifted as much to the south as it can possibly be, while the southern observers will see the path shifted as far as possible toward the north.

One thing, however, is to be remembered. A transit lasts several hours, and our observers must be so placed that the sun will not set during these hours. This consideration sometimes involves a difficulty. For our earth does not supply observing room all over her surface, and the very region where observation would be most serviceable may be covered by a widely-extended ocean. Then, again, the observing parties are being rapidly swayed round by the rotating earth; and it is often difficult to fix on a spot which may not, through this cause, be shifted from a favorable position at the beginning of the transit to an unfavorable one at the end.

Without entering on all the points of difficulty involved by such considerations as these, we may simply indicate the fact that the astronomer has a problem of considerable complexity to solve in applying Halley's mode of observation to a transit of Venus.

It was long since pointed out by the French astronomer Delisle that the subject may be attacked another way — that, in fact, instead of noticing how much longer the transit lasts in some places than in others, the astronomer may inquire how much earlier it begins or ends in some places than in others.

Here is another artifice, extremely simple in principle, though not altogether so simple in its application. Our readers must bear with us while we briefly describe the qualities of this second method, because in reality the whole question of the transit and all the points which have to be attended to in the equipment and placing of the various observing parties depend on these preliminary matters. Without attending to them — or at least to such primary points as we shall select — it would be impossible to form a clear conception of the circumstances with which astronomers are about to deal. There is, however, no real difficulty about this part of the subject, and we shall only ask of the reader to give his attention to it for a very brief space of time.

Suppose the whole of that hemisphere of the earth on which the sun is shining when the transit is about to begin were covered with observers waiting for the event. As Venus sweeps rapidly onward to the critical part of her path, it is clear that some of these observers will get an earlier view of the commencement of the transit than others will; just as at a boat-race, persons variously placed round a projecting corner of the course see the leading boat come into view at different times. Some one observer on the outer rim of the hemisphere would be absolutely the first to see the transit begin. Then rapidly other observers would see the phenomenon; and in the course of a few minutes some one observer on the outer rim of the hemisphere — almost exactly opposite the first — would be absolutely the last to see the transit begin. From that time the transit would be seen by all for several hours — we neglect the earth's rotation, of course — but the end of the transit, like the beginning, would not be seen simultaneously by the observers. First one would see it, then in succession the rest, and last of all an observer almost exactly opposite the first.

Now, here we have had to consider four observers who occupy exceptional positions. There is (1) the observer who sees the transit begin earliest, (2) the one who sees it begin latest, (3) the one who sees it end earliest, and (4) the one who sees it end latest. Let us consider the first two only. Suppose these two observers afterward compared notes, and found out what was the exact difference of time between their respective observations. Is it not clear that the result would at once afford the means of determining the sun's distance? It would be the simplest of all possible astronomical problems to determine over what proportion of her orbit Venus passed in the interval of time which elapsed between these observations; and the observers would now have learned that that portion of Venus's orbit is so many miles long, for they know what distance separated them, and it would be easy to calculate how much less that portion of Venus's orbit is. Thus they would learn what the length of her whole orbit is, thence her distance from the sun, and thence the sun's distance from us.

The two observers who saw the transit end earliest and latest could do the like.

Speaking generally, and neglecting all the complexities which delight the soul of the astronomer, this is Delisle's method of utilizing a transit. It has obviously one serious disadvantage as compared with the other. An observer at one side of the earth has to bring his observations into comparison with those made by an observer at the other side of the earth. Each uses the local time of the place at which he observes, and it has been calculated that for the result to be of value there must not be an error of a single second in their estimates of local time. Now, does the reader appreciate the full force of this proviso? Each observer must know so certainly in what exact longitude he is, that his estimate of the time when true noon occurs shall not be one second wrong! This is all satisfactory enough in places where there are regular observatories. But matters are changed when we are dealing with such places as Woahoo, Kerguelen Land, Chatham Island, and the wilds of Siberia.

Here, however, as in so many other cases, the astronomer must take what he can get and be thankful. If Nature insists on not revealing her secrets unless astronomers will betake themselves to all manner of desert and uncanny places, all astronomers can do is to face with boldness the difficulties thus placed in their way, and to do their utmost to bring them into complete subjection.

In the coming transit there are many such difficulties to be encountered. In fact, it is almost impossible to conceive a transit the circumstances of which are more inconvenient. On the other hand, however, the transit is of such a nature that if once the preliminary difficulties are overcome, we can hope more from its indications than from those of any other transit which will happen in the course of the next few centuries.

The transit will begin earliest for observers in the neighborhood of the Sandwich Islands, latest for observers near Crozet Island, far to the southeast of the Cape of Good Hope. It ends earliest for observers far to the southwest of Cape Horn, latest for observers in the northeastern parts of European Russia. Thus we see that, so far as the application of our second method is concerned, the suitable spots are not situated in the most inviting regions of the earth's surface. As the transit happens on December 8, 1874, the principal northern stations will be very bleak abodes for the observers. The southern stations are in yet more dreary regions — notwithstanding the fact that the transit occurs during the summer of the southern hemisphere.

For the application of Halley's method we require stations where the whole transit will be visible, and, as the days are very short at the northern stations in December, it is as respects these that we encounter most difficulty. However, it has been found that many places in Northern China, Japan, Eastern Siberia, and Mantchooria are suitable for the purpose. The best southern stations for this method lie, unfortunately, on the unexplored Antarctic Continent and the islands adjacent to it; but Crozet Island, Kerguelen Land, and some other places more easy of access than the Antarctic Continent, will serve very well. Indeed, England has so many stations to occupy elsewhere that it is doubtful whether she will care to undertake the dangerous and difficult task of exploring the Antarctic wastes to secure the best southern stations. The work may fairly be left to other nations, and doubtless will be efficiently carried out.

What England will actually undertake has not yet been fully decided upon. We may be quite certain that she will send out a party to Woahoo or Hawaii to observe the accelerated commencement of the transit. She will also send observers to watch the retarded commencement, but whether to Crozet Island, Kerguelen Land, Mauritius, or Rodriguez, is uncertain. Possibly two parties will be sent out for this purpose, and most likely Crozet Island and Mauritius will be the places selected. It had been thought until lately that the sun would be too low at these places when the transit begins, but a more exact calculation of the circumstances of the transit has shown this to be a mistake. Both Crozet Island and Kerguelen Land are very likely to be enveloped in heavy mists when the transit begins — that is, soon after sunrise — hence the choice of Mauritius or Rodriguez as a secondary station.

England will also be called on to take an important part in observing the accelerated end of the transit. A party will probably be sent to Chatham Island or Campbell Island, not far from New Zealand. It had been thought that at the former island the sun would be too low; but here, again, a more exact consideration of the circumstances of the transit has led astronomers to the conclusion that the sun will be quite high enough at this station.

The Russian observers are principally concerned with the observation of the retarded end of the transit, nearly all the best stations lying in Siberia. But there are several stations in British India where this phase can be very usefully observed; and doubtless the skilful astronomers and mathematicians who are taking part in the survey of India will be invited — as at the time of the great eclipse — to give their services in the cause of science. Alexandria, also, though inferior to several of the Indian stations, will probably be visited by an observing party from England.

It will be seen that England will thus be called on to supply about half a dozen expeditions to view the transit. All of these will be sent out in pursuance of Delisle's mode of utilizing a transit, so that, for reasons already referred to, it will be necessary that they should be provided with instruments of the utmost delicacy, and very carefully constructed.* They will have to remain at their several stations for a long time before the transit takes place — several months, at least — so that they may accurately determine the latitude of the temporary observatories they will erect. This is a work requiring skilled observers and recondite processes of calculation. Hence it is that the cost of sending out these observing parties is so considerable.

The only English party which will apply Halley's method of observation is the one which will be stationed at Crozet Island or Kerguelen Land. This part of their work will be comparatively easy, the method only requiring that the duration of the transit should be carefully timed. In fact, one of the great advantages of Halley's method is the smallness of the expense it involves. A party might land the day before the transit and sail away the day after, with results at least as trustworthy as those which a party applying Delisle's method could obtain after several months of hard work. It is to this, rather than any other cause, that the small expense of the observations made in 1769 is to be referred. And doubtless had it been decided by our astronomical authorities to apply Halley's method solely or principally, the expense of the transit-observations would have been materially lessened. There would, however, have been a risk of failure through the occurrence of bad weather at the critical stations; whereas now—as other nations will doubtless avail themselves of Halley's method — the chance that the transit-observations will fail through meteorological causes is very largely diminished. Science will owe much to the generosity of England in this respect.

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* It is held to be of the utmost importance that all the observing parties should use similar telescopes. It would be well if the class of telescope selected were Browning's six-inch reflectors.

It is, indeed, only recently that the possibility of applying Halley's method has been recognized. It had been thought that the method must fail totally in 1874. But on a more careful examination of the circumstances of the transit, a French astronomer, M. Puiseux, was enabled to announce that this is not the case. Almost simultaneously the present writer published calculations pointing to a similar result; but having carried the processes a few steps further than M. Puiseux, he was able to show that Halley's method is not only available in 1874, but is the more powerful method of the two.

Unfortunately, there is an element of doubt in the inquiry, of which no amount of care on the part of our observers and mathematicians will enable them to get rid. We refer to the behavior of Venus herself. It is to the peculiarity we are now to consider that the *quasi*-failure of the observations made in 1769 must be attributed. It is true that Mr. Stone, the eminent first-assistant at the Greenwich Observatory, has managed to remove the greater part of the doubts which clouded the results of those observations. But not even his skill and patience can serve to remove the blot which a century of doubt has seemed to throw upon the most exact of the sciences. We shall now show how much of the blame of that unfortunate century of doubt is to be ascribed to Venus.

At a transit, astronomers confine their attention to one particular phase — the moment, namely, when Venus just seems to lie wholly within the outline of the sun's disk. This at least was what Halley and Delisle both suggested as desirable. Unfortunately, Venus had not been consulted, and when the time of the transit came she declined to enter upon or leave the sun's face in the manner suggested by the astronomers. Consider, for example, her conduct when entering on the sun's face:

At first, as the black disk of the planet gradually notched the edge of the sun's disk, all seemed going on well. But when somewhat more than half of the planet was on the sun's face, it began to be noticed that Venus was losing her rotundity of figure. She became gradually more and more pear-shaped, until at last she looked very much like a peg-top touching with its point the edge of the sun's disk. Then suddenly —"as by a lightning-flash," said one observer — the top lost its peg, and then gradually Venus recovered her figure, and the transit proceeded without further change on her part until the time came for her to leave the sun's face, when similar peculiarities took place in a reversed order.

Here was a serious difficulty indeed. For when was the moment of true contact? Was it when the peg-top figure seemed just to touch the edge of the sun? This seemed unlikely, because a moment after the planet was seen well removed from the sun's edge. Was it when the rotund part of the planet belonged to a figure which would have touched the sun's edge if the rotundity had been perfect elsewhere? This again, seemed unlikely, because at this moment the black band connecting Venus and the sun was quite wide. And, besides, if this were the true moment of contact, what eye could be trusted to determine the occurrence of a relation so peculiar? Yet the interval between this phase and the final or peg-top phase lasted several seconds — as many as twenty-two in one instance in 1769 — and the whole success of the observation depended on exactness within three or four seconds at the outside.

We know that Venus will act in precisely the same manner in 1874. If we had been induced to hope that improvements in our telescopes would diminish the peculiarity, the observations of the transit of Mercury in November, 1868, would have sufficed to destroy that hope, for, even with the all but perfect instruments of the Greenwich Observatory, Mercury assumed the peg-top disguise in the most unpleasing manner.

It may be asked, then, What do astronomers propose to do in 1874 to prevent Venus from misleading them again as she did in 1769? Much has already been done toward this end. Mr. Stone undertook a series of careful researches to determine the law according to which Venus may be expected to behave or to misbehave herself; and the result is, that he has been able to tell the observers exactly what they will have to look for, and exactly what it is most important that they should record. In 1769, observers recorded their observations in such doubtful terms, owing to their ignorance of the real significance of the peculiarities they witnessed, that the mathematicians who had to make use of those observations were misled. *Hinc* *illæ lacrymæ.* Hence it is that an undeserved reproach has fallen upon the "exact science."

The amount of the error resulting from the misinterpretation of the observations made in 1769 was, however, very small indeed, when its true character is considered. It is, indeed, easy to make the error seem enormous. The sun's distance came out some four millions of miles too large, and that seems no trifling error. Then, again, the resulting estimate of the distance of Neptune came out more than a hundred million miles too great; while even this enormous error was as nothing when compared with that which resulted when the distances of the fixed stars were considered.

But this is an altogether erroneous mode of estimating the effect of the error. It would be as absurd to count up the number of hairs' breadth by which the geographer's estimates of the length and breadth of England may be in error. In all such matters it is relative and not absolute error we have to consider. A microscopist would have made a bad mistake who should over-estimate the length of a fly's proboscis by a single hair's breadth; but the astronomer had made a wonderfully successful measurement of the sun's distance who deduced it within three or four millions of miles of the true value. For it is readily calculable that the error in the estimated relative bearing of the sun as seen from opposite sides of the earth corresponds to the angle which a hair's breadth subtends when seen from a distance of 125 feet.

The error was first detected when other modes of determining the sun's distance were applied by the skilful astronomers and physicists of our own day. We have no space to describe as fully as they deserve the ingenious processes by which the great problem has been attacked without aid from Venus. Indeed, we can but barely mention the principles on which those methods depend. But to the reader who takes interest in astronomy, we can recommend no subject as better worth studying than the masterly researches of Foucault, Leverrier, Stone, and Hansen, upon the problem of the sun's distance.

The problem has been attacked in four several ways. First, the tremendous velocity of light has been measured by the ingenious arrangement of revolving mirrors; the result combined with the known time occupied by light in travelling across the earth's orbit immediately gives the sun's distance. Secondly, a certain irregularity in the moon's motion, due to the fact that she is most disturbed by the sun when traversing that half of her path which is nearest to him, was pressed into the service with similar results. Thirdly, an irregularity in the earth's motion, due to the fact that she circles around the common centre of gravity of her own mass and the moon's, was made a means of attacking the problem. Lastly, Mars, a planet which, as we have already mentioned, approaches us almost as nearly as Venus, was found an efficient ally.

The result of calculations founded on these methods showed that the sun's distance, instead of being about 95,000,000 miles, is little more than 91,500,000 miles. And recently, by a careful reëxamination of the observations made upon Venus in 1769, Mr. Stone has shown that they point to a similar result.

Doubtless, however, we must wait for the transit of Venus in 1874 before forming a final decision as to the estimate of the sun's distance which is to take its place in popular works on astronomy during the next century or so. Nothing but an unlooked-for combination of unfavorable circumstances can cause the failure of our hopes. Certainly, if we should fail in obtaining satisfactory results in 1874, the world will not say that the generosity of the English Government has been in fault, since it would be difficult to find a parallel in the history of modern science to the munificence of the grant which has been made this year for expeditions to observe a phenomenon whose interest and importance are purely scientific.

RECENT SOLAR RESEARCHES.

Since the great eclipse of August, 1868, our knowledge respecting the constitution of the sun has been steadily progressing. One discovery after another has been made, and there really seems to be no reason for believing that we have as yet nearly reached the limits of the knowledge which spectroscopic analysis is capable of supplying. Indeed, the invention of a new form of spectroscope — the ingenious automatic spectroscope of Mr. Browning — promises soon to be rewarded by a series of discoveries as important as any which have hitherto been made. We propose briefly to indicate the present position of our knowledge respecting the great central luminary of our system.

The spectroscopic observation of the eclipse of August, 1868, had shown that the strange prominences seen during total eclipses of the sun are vast masses of luminous vapor — hydrogen-flames, we may call them, considering how largely hydrogen enters into their constitution. Only we must remember that it is hydrogen glowing from intensity of heat simply, and not burning hydrogen, that constitutes these prominences. Now, it had long been recognized that the colored prominences spring from an envelope of a similar nature surrounding the whole surface of the sun. Father Secchi, of the Collegio Romano, in a lecture given to the pupils of the École Ste. Geneviève, had thus in 1867 described this envelope (whose existence he was the first to recognize): "The observation of eclipses furnishes indisputable evidence that the sun is really surrounded by a layer of red matter, of which we commonly see no more than the most elevated points." One of the first and most interesting results of the eclipse-observations was Mr. Lockyer's confirmation of the justice of this opinion. He and Jannsen had independently shown that the existence of prominences can be recognized when the sun is not eclipsed; and the same method supplied clear evidence of the existence of this red envelope, to which Mr. Lockyer gave the name of the *chromosphere*. Remembering who first indicated its existence as "indisputable," we may conveniently call it Secchi's chromosphere.

Both the chromosphere and the prominences consist of glowing vapor. But there is a difference in their constitution. In the prominences there are usually but very few constituent vapors. Hydrogen is there, and another vapor, whose nature is as yet undetermined, while occasionally there are the vapors of other elements. But in the chromosphere there are commonly several elements, and sometimes there are many.

Here, then, we have above the photosphere of the sun a vaporous envelope, obviously of a complicated structure, and perhaps far more complicated than it has yet been proved to be. For it must be remembered that the lowest layers of this envelope might be composed of the vapors of numerous elements, and yet no record of their existence be recognized. A depth of ten miles would correspond to so small a portion of the sun's diameter (about the 85,000th part) as to be wholly unrecognizable by any telescopic power men can hope to obtain. If any of our readers are telescopists, they will know what force lies in the remark that such a distance would subtend about the 44th part of a second of arc, so that no less than twenty-six such distances could be placed between the components of that well-known test-object, the double companion of the star Gamma Andromedæ.*

Next below this colored envelope there is the mottled photosphere, either a white-hot surface with relatively dark pores all over it, or, according to other and better authorities, a surface of white-hot spots spread over a relatively dark background. Here we are describing merely its appearance; what the constitution of this surface may in reality be remains yet to be determined.

Beneath the photosphere there are vast depths of vapor, for when the photosphere is broken through where spots are formed, the spectroscope tells us that the relatively dark regions thus disclosed are filled with the vapors of various elements. We know that the dark lines which cross the rainbow-tinted solar spectrum are caused by the light-absorbing action of the vapors which surround the sun, and these lines are seen more distinctly in the spectrum of a sun-spot than in that of the photosphere.

Now, it is worthy of notice that all that has thus far been discovered tends to confirm the theory put forward nearly a century ago by Sir William Herschel. That thoughtful observer recognized in the solar photosphere a widely-extended layer of luminous clouds, while he regarded the light of the penumbræ of sun-spots as coming from a lower cloud-layer. He conceived that up-rushes of vapor, thrusting aside both layers, caused the appearance of a solar spot. We have heard a great deal lately of the English and Continental theories of the solar constitution; but the evidence we have recently obtained goes far to show that, after all, Sir William Herschel, without the aid of spectroscope or polariscope, formed a juster view of the solar constitution than any which has been recently propounded. He was doubtless mistaken in the view (which he put forward as a mere hypothesis) that the real surface of the sun may be not very intensely heated. We have every reason to believe that the whole mass of the sun is raised to an inconceivable degree of heat. But for the rest, there seems far more reason to believe in Sir William Herschel's cloud-layer theory than in any other which has been put forward in recent times.

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* The view here presented was completely confirmed during the eclipse of last December. Professor Young and Mr. Pye independently recognized a layer whose spectrum showed all the Fraunhofer lines reversed. By observing at the place where the moon had just concealed the last fine sickle of the solar disk, they obviated the effects of diffraction, which render the observation wholly impossible in the case of the uneclipsed sun.

Let us consider some of the consequences of such a constitution. Imagine the ascent of vapors of many elements from the fluid surface of the solar oceans. This mixed atmosphere is in reality aglow with the intensest heat and light, so that, if we could examine its spectrum separately, we should see the bright lines of the various vaporous elements which constitute it. But intensely hot as it is, it must yet be less hot than the surface from which it has risen, because the formation of vapors is a process in which heat is used up. And therefore, by a well-known law, the spectrum of the light from the white-hot surface shining through the atmosphere will be a rainbow-tinted streak, crossed by the dark lines corresponding to the various elements composing that atmosphere. But as the lighter vapors in this mixed atmosphere ascend, they reach a region of less pressure, and a region where they can part more freely with their heat. Thus, precisely as the cumulus-clouds form in our own atmosphere, so would a layer of clouds be formed somewhat low down in the solar atmosphere. But from the upper surface of this layer the vapors of the elements composing the clouds would rise, again to condense at a higher level, much as the light cirrus-clouds in our own atmosphere form at a great height above the layer of cumulus-clouds.

The great difference between this process and what takes place in our own atmosphere would consist in the fact that whereas the only kind of cloud which can form in our air is a water-cloud, there can be formed in the solar atmosphere clouds of iron, copper, zinc, and other such elements, each element having its own distinct range, so to speak, within the limits of the solar atmosphere.

Now, with such processes as these going on, we can conceive how rushes of heated gas might from time to time thrust aside the cloud-layers; and how where this happened we should occasionally recognize the bright lines corresponding to the more intensely-heated gas, as well as the dark lines corresponding to the deep vapor-masses laid bare by the removal of the photosphere. And precisely in this way do the observations recently made by Mr. Lockyer seem alone to be explicable. He sees the glowing vapors above the photosphere stirred from time to time as by fierce tempests — nay, he is enabled to measure (very roughly, of course) the velocity with which these solar winds urge their way through the chromosphere itself, in the neighborhood of these spots. The progress of these hurricanes is often indicated by the appearance of bright lines in those parts of the spectrum where usually dark lines are seen.

Truly Kirchhoff's discovery of the significance of the spectral lines is bearing wonderful fruit! Who would have thought that researches carried on with a few triangular prisms of glass on the light from such a substance as sodium, the basis of our commonplace soda, would lead to the result that solar tornadoes could be watched as readily with the spectroscope as in Galileo's time the sun-spots themselves could be traced across the sun's disk with the telescope?*

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* I give this paper as it appeared in the *Spectator*. But there are some points requiring correction. In the first place, the objectionable word *chromosphere* (for *chromatosphere*) should be replaced by ***sierra***. Secondly, there is an error as to the absolute priority of Secchi in recognizing the sierra. He went considerably beyond all others in the matter, having not only reasoned upon but seen and photographed the sierra, and having furthermore found evidence as to its nature when studying sun-spots. But Professors Grant and Swan, as well as Von Littrow, the Imperial Astronomer of Austria, had recognized the existence of the sierra before Secchi, and Leverrier had also independently arrived at the same conclusion as Secchi, and at about the same time. I had not known of some of these claims and had forgotten others when I wrote the above paper. This will scarcely seem surprising when it is remembered that the views of Grant, Swan, Von Littrow, and Leverrier, had not been made widely public — as Secchi's had — by being published in popular treatises and in lectures. It was with some surprise, therefore, that I found myself charged, not only with ignorance, but, incongruously enough, with injustice also, by a fellow-worker in astronomy, who addressed a letter to the editor of the *Spectator*, advocating in needlessly warm terms the prior claims of Grant and Swan. It is perhaps unnecessary for me to say that the charge of injustice was wholly undeserved; and I think the writer of the letter would have inferred this had he considered a parallel instance which had recently occurred. For a well-known worker had claimed the very same discovery only a few months before as *his own;* and, although the subject was specially his, he had

GOVERNMENT AID TO SCIENCE.

AMONG the questions which will occupy the attention of the new Parliament, we think we may safely include, in anticipation, the subject of State intervention to secure the progress of physical science.* It will be remembered that this subject was brought before the notice of the British Association, at its recent meeting, by Lieutenant-Colonel Strange, and a committee — including the names of Professors Sir William Thomson, Tyndall, Frankland, Williamson, Stokes, Fleming, Jenkins, Hirst, and Huxley, Messrs. Glaisher and Huggins, and Drs. Stenhouse, Balfour Stewart, and Mann — was appointed to consider and report upon the subject. Science has now reached a peculiar stage in that long and remarkable career of progress which was inaugurated toward the close of the sixteenth century. Hitherto those who have been able and willing to take part in scientific researches have had the means of doing so without incurring great expense, and many have even found it possible to do good and useful service in the cause of science while prosecuting, at the same time, the labors of their profession or trade. But now the case is very different. A man who would assist in forwarding the progress of science must give his whole energies to the cause; he must be prepared to incur large expenses; and all this he must do without the hope that science will make him any pecuniary return. Theoretically, indeed, it may be argued that he will labor best who hopes for no return for his labors; who works, not for profit, but from pure love of science, and so on. But, as a matter of fact, many of those who would serve science best, and hundreds of those who could do yeoman's service in her cause, are simply debarred from

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not known even of Secchi's numerous public statements respecting the sierra, yet no one thought of charging him with injustice. The writer of the letter could scarcely have forgotten the circumstances, since that worker was no other than himself.

* The reader need hardly be told that the hopes here expressed were completely disappointed.

scientific pursuits by the necessity of earning the means of subsistence. And there are crowds of others who, though they may be independent in means, are yet unable to provide themselves with the expensive instruments by which alone any useful work can now be done. For, as Colonel Strange observes, "Science can no longer be cultivated as in by-gone times it used to be. In astronomy the man with his table spy-glass cannot now furnish acceptable results. In chemistry, the Wollaston tea-tray and wine-glasses are superseded by well-equipped laboratories. In optics we see elaborate spectroscopes, not Newton's simple prism. In meteorology, and in every investigation of continuous phenomena, we are satisfied with nothing less than self-recording instruments. In electricity, in microscopy, and in other branches, our appliances are every day more and more amplified. The age of great discoveries made, and, above all, extensive series of facts accumulated with limited means, is passing away; and we are every day compelled to employ more perfect appliances and more systematic agencies in unravelling the secrets of Nature."

It is scarcely necessary to point out that the aid of the State in securing the progress of physical science is not asked without the promise of a *quid pro quo*. It is not as though the State were called upon to aid in antiquarian, or entomological, or numismatic researches, or in any subject of inquiry which, however interesting, has no practical bearing on the wants of daily life, or on the appliances by which the social state of man may be benefited and improved. Nor is it to secure the spread of scientific knowledge that State aid is called for, but to secure the progress of physical science. That that progress cannot fail to bring with it important advantages to mankind it is almost needless to assert. We have only to look around us to see what science has done for mankind. But those are best acquainted with the practical value of scientific knowledge who are themselves engaged in scientific researches, or are at least proficient in scientific matters. Hundreds, for example, might see in the complicated instruments which are to be found in the Greenwich Observatory nothing but ingenious applications for the solution of theoretical problems; it is only astronomers, or those who are versed in the processes of astronomy, who know that our whole system of commerce would be affected injuriously if those instruments were destroyed or left unused. Here we have an instance of science working under State patronage, working in the cause of the State; and what Colonel Strange proposes is to multiply instances of this sort. The State profits by the labors of the Greenwich astronomers, and those astronomers would for the most part be unable or unwilling to continue their labors but for the pecuniary reward which they receive from the State. But assuredly the State would suffer more than the astronomers if the establishment at Greenwich were done away with. And precisely in the same way the State would reap important advantages from the labors of proficients in other departments of science who are now debarred by considerations of expense, or by the necessity of earning a livelihood, from applying their skill to forward the cause of scientific progress.

Colonel Strange's proposal includes the establishment of national institutions expressly for the practical advancement of scientific research. He remarks that "men engaged in science need hardly be told that when they discover a new substance, the determination of the physical properties of which is attended with cost and labor, they experience a great — perhaps insuperable — difficulty in obtaining its examination. A new theory, or the confutation or confirmation of an old one, if dependent on any considerable accumulation of facts, shares even a worse fate." Important benefits could not fail to result if difficulties such as these were removed from the paths of physical research, by the institution of bodies whose duty it would be to undertake, and complete in an accurate and systematic manner, costly and tedious investigations on which vast interests may be dependent.

AMERICAN ALMS FOR BRITISH SCIENCE.*

Our astronomers have received an invitation which is as pleasing to them as men of science as it is painful to them as Englishmen. As our readers know, sixty-eight persons had volunteered to go to Spain and Sicily to view the total eclipse of December 22d; our scientific societies had voted large sums of money for the equipment of the two observing parties; and every one was certain that Government would supply the means of transport. But every one was mistaken. The Admiralty discovered that the nation would assuredly disapprove if room were found for mere men of science and their trumpery in any of Her Majesty's ships; and accordingly, just when the extensive preparations requisite for the expeditions were in full progress, news came that the means of transport must be found by the observers themselves. We do not care here — we hardly have patience, indeed — to discuss the probable cause of a refusal so discreditable to the scientific repute of England. It had been announced by the Astronomer-Royal (in connection with another matter), that Government would always be found liberal in scientific matters, if a sufficient cause were shown by persons in whom they had trust; and we do not care to inquire whether the Astronomer-Royal was mistaken in this matter, or whether the Government declined to put trust in him or in the Presidents of our Astronomical and Royal Societies, or whether, lastly, the sufficient cause was not brought before the Government with proper earnestness. Let the explanation be what it may, the fact remains — England has been exhibited to all the nations as turning her back on science, and English men of science have been discredited before the world as unworthy of England's confidence.

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* This was one of a series of articles which appeared in the *Daily News* during the months which followed the announcement that the British Government would give no aid to the eclipse expedition. To the liberality with which the *Daily News* gave space for these appeals may

But now news comes that the Government of the United States has not only found means of transport for two American parties, but has made the handsome grant of £6,000, to furnish suitable appliances for observing the eclipse. The American men of science have reached England. They recognize the pitiable condition to which our astronomers have been reduced by the Government, and they invite our sixty-eight volunteers to sail with them. A letter has been sent to these volunteers,

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fairly be ascribed the fact that eventually the eclipse committee was aroused to something like energetic action. When the real state of the case became known to Government, ample assistance was rendered. The shortness of the time eventually left for preparation may be regarded as accounting for subsequent seeming short-comings on the part of the Organizing Committee; while fortunately the zeal of the expeditionists averted the risk (which at one time seemed serious) that rather brusque usage would cause some of the most important members of the expeditions to withdraw their aid.

inviting them, in the name of the American expeditionary parties, to accept this much-needed assistance. The offer is most generous; it is most inviting; it is one which no astronomer is justified in declining on account of sentimental considerations. But it certainly is a new and a painful position for an English man of science to be placed in, thus to find scientific alms offered him as a reparation for the insult he has, in effect, received from his own Government.

Many may be disposed to wonder why so much interest is attached to this particular eclipse. During many former total eclipses — even when they have been visible at more conveniently accessible stations — less care was taken to fit out expeditions. And, what is even more to the point, observations have been made on eclipse after eclipse, in former times, without adding jot or tittle to our knowledge of solar physics. But during recent eclipses things have altered. In 1860 the celebrated "Himalaya Expedition" sailed to Spain from England; while other parties came from France, Italy, and Germany. And, though the old fault of wasting observing energy on matters already known or demonstrated prevailed very largely, yet De la Rue and Secchi, by photographing the eclipsed sun, well repaid the whole cost of these expeditions. In the great total eclipse of August, 1868, Europe sent out many observing parties to India, and the great discovery that the red prominences seen round the totally-eclipsed sun are masses of glowing vapor sufficiently repaid the cost. In August, 1869, the Americans availed themselves right skillfully and worthily of the passage of the moon's shadow across their continent; and, though they failed in the main purpose they had set themselves, they made preliminary observations of the utmost importance and value. That purpose was to ascertain the nature of the glorious aureole of light seen around the sun during total eclipses; and it is with the same purpose that the expeditions formed for observing the present eclipse were to have set forth. The questions to be answered are full of interest, even now when their full significance is not known, while it may well be that when we begin to have accurate information about them, we shall find they have a real importance wholly unlooked for. As the last direct rays of the sun are concealed by the advancing moon, there springs into view a glorious crown of colored light — pearly white in parts, faintly pink beyond, and at the extreme verge showing tints of mauve and violet and green — delicate and beautiful beyond description. Through this coronal glory there extend rays of bluish-white light, reaching often to a vast distance from the black disk of the moon. Commonly remaining unchanged in position, these rays sometimes — if all the narratives can be trusted — exhibit very obvious signs of motion, resembling in this respect those streamers of colored light which we have lately so much admired in the aurora. Indeed, wonderful as it may seem, the corona has lately come to be regarded as associated in some way with the Aurora Borealis. We know that those auroral streamers which form so wonderful a display in our own skies are due to solar influences. In whatever way it may be brought about, certain it is that disturbances of the sun are reflected in terrestrial auroral displays. The auroras which have occurred lately were predicted by astronomers, who know that the sun is undergoing during the present year disturbances of the most amazing nature. Solar spots, of various dimensions, have been counted by the hundred of late; and we now know that when the sun is thus spotted our earth sympathizes with the central orb. Thrilling from pole to pole in magnetic tremor, she spreads out over both hemispheres the auroral banners that indicate the progress of electric revolutions. The devices of her children for utilizing her electric forces are for the time set at naught, and the telegraph-clerk finds for a while that Mother Earth is having her own way and will not obey his behests. If the sun, ninety millions of miles away from us, thus affects the earth's frame, and thus illuminates terrestrial skies, it need not be greatly wondered at should it be proved that he illuminates with no dissimilar light the regions lying more closely around him. If there are no planets like our earth in these regions, no large bodies on which the sun can exert his inconceivable powers, there are yet in these spaces — unless astronomers are at fault — uncounted millions of minute bodies, those tiny "pocket-planets" which pass at times through our own atmosphere, and are called by us falling stars, or meteors. Among these tiny bodies auroral gleams may pass, producing by their united lustre the glories of the solar corona.

But, whether this view be just, or whether, as Mr. Lockyer holds, the corona is only a phenomenon of our own air, or is due (as the fanciful M. Faye once thought of the colored prominences) to some sort of lunar mirage, certain it is that just now it is a matter of extreme interest that further observations should be made. Undoubtedly, what we have lately learned respecting the sun gives an interest and importance to this matter of the solar corona which it never before possessed. Yet this is the problem respecting which our Government is understood to have said to astronomers, "As far as we can, we will prevent you from solving it."

Truly it would be difficult to show that any material profit can be gained by solving the problems associated with the solar corona. The tree of science has its blossoms as well as its fruits, and perhaps the results of the observations we are advocating will belong to the former rather than the latter. But what then? Can we limit science to remunerative researches alone? As well might we attempt to get fruit from a tree whose leaves and blossoms we systematically plucked off. Latent though the power of science now is in great part, yet science is the greatest power our country possesses. It has been treated for a long while as a troublesome beggar — a few hundreds doled out here and a few thousands there. The country does not yet know its own interest. Because little has been asked, it has thought little could be returned. The time is coming when not hundreds or thousands will be asked for science, but millions freely and eagerly given — when the example of other countries, rapidly passing in advance of England through their scientific resources, will force on our attention the folly of a system which grants thirty millions yearly to secure the means of carrying on war, and refuses a few paltry thousands to secure the noblest portion of our strength.

THE SECRET OF THE NORTH POLE.

If an astronomer upon some distant planet has ever thought the tiny orb we inhabit worthy of telescopic study, there can be little doubt that the snowy regions which surround the arctic and antarctic poles must have attracted a large share of his attention. Waxing and waning with the passing seasons, those two white patches afford significant intelligence respecting the circumstances of our planet's constitution. They mark the direction of the imaginary axial line upon which the planet rotates; so that we can imagine how an astronomer on Mars or Venus would judge from their position how it fares with terrestrial creatures. There may, indeed, be Martial Whewells who laugh to scorn the notion that a globe so inconveniently circumstanced as ours can be inhabited, and are ready to show that if there were living beings here they must be quickly destroyed by excessive heat. On the other hand, there are doubtless skeptics on Venus also who smile at the vanity of those who can conceive a frozen world, such as this our outer planet, to be inhabited by any sort of living creature. But we doubt not that the more advanced thinkers both in Mars and Venus are ready to admit that, though we must necessarily be far inferior beings to themselves, we yet manage to "live and move and have our being" on this ill-conditioned globe of ours. And these, observing the earth's polar snowcaps, must be led to several important conclusions respecting physical relations here.

It is, indeed, rather a singular fact to contemplate, that exterrestrial observers, such as these, may know much more than we ourselves do respecting those mysterious regions which lie close around the two poles. Their eyes may have rested on spots which with all our endeavors we have hitherto failed to reach. Whether, as some have thought, the arctic pole is in summer surrounded by a wide and tide-swayed ocean; whether there lies around the antarctic pole a wide continent bespread with volcanic mountains larger and more energetic than the two burning cones which Ross found on the outskirts of this desolate region; or whether the habitudes prevailing near either pole are wholly different from those suggested by geographers and voyagers — such questions as these might possibly be resolved at once, could our astronomers take their stand on some neighboring planet, and direct the searching power of their telescopes upon this terrestrial orb. For this is one of those cases referred to by Humboldt, when he said that there are circumstances under which man is able to learn more respecting objects millions of miles away from him than respecting the very globe which he inhabits.

If we take a terrestrial globe, and examine the actual region near the North Pole which has as yet remained unvisited by man, it will be found to be far smaller than most people are in the habit of imagining. In nearly all maps the requirements of charting result in a considerable exaggeration of the polar regions. This is the case in the ordinary "maps of the two hemispheres" which are to be found in all atlases. And it is, of course, the case to a much more remarkable extent in what is termed Mercator's projection. In a Mercator's chart we see Greenland, for example, exaggerated into a continent fully as large as South America, or to seven or eight times its real dimensions.

There are three principal directions in which explorers have attempted to approach the North Pole. The first is that by way of the sea which lies between Greenland and Spitzbergen. We include under this head Sir Edward Parry's attempt to reach the pole by crossing the ice-fields which lie to the north of Spitzbergen. The second is that by way of the straits which lie to the west of Greenland. The third is that pursued by Russian explorers who have attempted to cross the frozen seas which surround the northern shores of Siberia.

In considering the limits of the unknown north-polar regions, we shall also have to take into account the voyages which have been made around the northern shores of the American Continent in the search for a "northwestern passage." The explorers who set out upon this search found themselves gradually forced to seek higher and higher latitudes if they would find a way round the complicated barriers presented by the ice-bound straits and islands which lie to the north of the American Continent. And it may be noticed in passing, as a remarkable and unforeseen circumstance, that the farther north the voyagers went the less severe was the cold they had to encounter. We shall see that this circumstance has an important bearing on the considerations we shall presently have to deal with.

One other circumstance respecting the search for the northwest passage, though not connected very closely with our subject, is so singular and so little known that we feel tempted to make mention of it at this point. The notion with which the seekers after a northwest passage set out was simply this, that the easiest way of reaching China and the East Indies was to pursue a course resembling as nearly as possible that on which Columbus had set out — if only it should appear that no impassable barriers rendered such a course impracticable. They quickly found that the American Continents present an unbroken line of land from high northern latitudes far away toward the antarctic seas. But it is a circumstance worth noticing, that if the American Continents had no existence, the direct westerly course pursued by Columbus was not only not the nearest way to the East-Indian Archipelago, but was one of the longest routes which could possibly have been selected. Surprising as it may seem at first sight, a voyager from Spain for China and the East Indies ought, if he sought the absolutely shortest path, to set out on an almost direct northerly route! He would pass close by Ireland and Iceland, and so, near the North Pole, and onward into the Pacific. This is what is called the great-circle route; and if it were only a practicable one, would shorten the course to China by many hundreds of miles.

Let us return, however, to the consideration of the information which arctic voyagers have brought us concerning the north-polar regions.

The most laborious researches in arctic seas are those which have been carried out by the searchers after a northwest passage. We will therefore first consider the limits of the unknown region in this direction. Afterward we can examine the results of those voyages which have been undertaken with the express purpose of reaching the North Pole along the three principal routes already mentioned.

If we examine a map of North America constructed in recent times, we shall find that between Greenland and Canada an immense extent of coast-line has been charted. A vast archipelago covers this part of the northern world. Or, if the strangely-complicated coast-lines which have been laid down really belong to but a small number of islands, the figures of these must be of the most fantastic kind. Toward the northwest, however, we find several islands whose outlines have been entirely ascertained. Thus we have in succession North Devon Island, Cornwallis Island, Melville Island, and Port Patrick Island, all lying north of the seventy-fifth parallel of latitude. But we are not to suppose that these islands limit the extent of our seamen's researches in this direction. Far to the northward of Wellington Channel, Captain de Haven saw, in 1852, the signs of an open sea — in other words, he saw, beyond the ice-fields, what arctic seamen call a "water-sky." In 1855 Captain Penny sailed upon this open sea; but how far it extends toward the North Pole has not yet been ascertained.

It must not be forgotten that the northwest passage has been shown to be a reality, by means of voyages from the Pacific as well as from the Atlantic. No arctic voyager has yet succeeded in passing from one ocean to the other. Nor is it likely now that any voyager will pursue his way along a path so beset by dangers as that which is called the northwest passage. Long before the problem had been solved, it had become well known that no profit could be expected to accrue to trade from the discovery of a passage along the perilous straits and the ice-encumbered seas which lie to the north of the American Continent. But Sir Edward Parry having traced out a passage as far as Melville Island, it seemed to the bold spirit of our arctic explorers that it might be possible, by sailing through Behring's Straits, to trace out a connection between the arctic seas on that side and the regions reached by Parry. Accordingly, McClure, in 1850, sailed in the "Investigator," and passing eastward, after traversing Behring's Straits, reached Baring's Land, and eventually identified this land as a portion of Banks's Land, seen by Parry to the southward of Melville Island.

It will thus be seen that the unexplored parts of the arctic regions are limited in this direction by sufficiently high latitudes.

Turn we next to the explorations which Russian voyagers have made to the northward of Siberia. It must be noticed, in the first place, that the coast of Siberia runs much farther northward than that of the American Continent. So that on this side, independently of sea explorations, the unknown arctic regions are limited within very high latitudes. But attempts have been made to push much farther north from these shores. In every case, however, the voyagers have found that the ice-fields, over which they hoped to make their way, have become gradually less and less firm, until at length no doubt could remain that there lay an open sea beyond them. How far that sea may extend is a part of the secret of the North Pole; but we may assume that it is no narrow sea, since otherwise there can be little doubt that the ice-fields which surround the shores of Northern Siberia would extend unbroken to the farther shores of what we should thus have to recognize as a strait. The thinning-off of these ice-fields, observed by Baron Wrangel and his companions, affords, indeed, most remarkable and significant testimony respecting the nature of the sea which lies beyond. This we shall presently have to exhibit more at length; in the meantime we need only remark that scarcely any doubt can exist that the sea thus discovered extends northward to at least the eightieth parallel of latitude.

We may say, then, that from Wellington Channel, northward of the American Continent, right round toward the west, up to the neighborhood of Spitzbergen, very little doubt exists as to the general characteristics of arctic regions, save only as respects those unexplored parts which lie within ten or twelve degrees of the North Pole. The reader will see presently why we are so careful to exhibit the limited extent of the unexplored arctic regions in this direction. The guess we shall form as to the true nature of the north-polar secret will depend almost entirely on this consideration.

We turn now to those two paths along which arctic exploration, properly so termed, has been most successfully pursued.

It is chiefly to the expeditions of Drs. Kane and Hayes that we owe the important knowledge we have respecting the northerly portions of the straits which lie to the west of Greenland. Each of these explorers succeeded in reaching the shores of an open sea lying to the northeast of Kennedy Channel, the extreme northerly limit of those straits. Hayes, who had accompanied Kane in the voyage of 1854-'55, succeeded in reaching a somewhat higher latitude in sledges drawn by Esquimaux dogs. But both expeditions agree in showing that the shores of Greenland trend off suddenly toward the east at a point within some nine degrees of the North Pole. On the other hand, the prolongation of the opposite shore of Kennedy Channel was found to extend northward as far as the eye could reach. Within the angle thus formed there was an open sea "rolling," says Captain Maury, "with the swell of a boundless ocean."

But a circumstance was noticed respecting this sea which was very significant. The tides ebbed and flowed in it. Only one fact we know of — a fact to be presently discussed — throws so much light on the question we are considering as this circumstance does. Let us consider a little whence these tidal waves can have come.

The narrow straits between Greenland on the one side, and Ellesmere Land and Grinnell Land on the other, are completely ice-bound. We cannot suppose that the tidal wave could have found its way beneath such a barrier as this. "I apprehend," says Captain Maury, "that the tidal wave from the Atlantic can no more pass under this icy barrier, to be propagated in the seas beyond, than the vibrations of a musical string can pass with its notes a fret on which the musician has placed his finger."

Are we to suppose, then, that the tidal waves were formed in the very sea in which they were seen by Kane and Hayes? This is Captain Maury's opinion: "These tides," says he, "must have been born in that cold sea, having their cradle about the North Pole."

But if we carefully consider the theory of the tides this opinion seems inadmissible. Every consideration on which that theory is founded is opposed to the assumption that the moon could by any possibility raise tides in an arctic basin of limited extent. It would be out of place to examine at length the principle on which the formation of tides depends. It will be sufficient for our purposes to remark that it is not to the mere strength of the moon's "pull" upon the waters of any ocean that the tidal wave owes its origin, but to the difference of the forces by which the various parts of that ocean are attracted. The whole of an ocean cannot be raised at once by the moon; but if one part is attracted more than another, a wave is formed. That this may happen, the ocean must be one of wide extent. In the vast seas which surround the Southern Pole there is room for an immensely powerful "drag," so to speak; for always there will be one part of these seas much nearer to the moon than the rest, and so there will be an appreciable difference of pull upon that part.

The reader will now see why we have been so careful to ascertain the limits of the supposed north-polar ocean, in which, according to Captain Maury, tidal waves are generated. To accord with his views, this ocean must be surrounded on all sides by impassable barriers either of land or ice. These barriers, then, must lie to the northward of the regions yet explored, for there is open sea communicating with the Pacific all round the north of Asia and America. It only requires a moment's inspection of a terrestrial globe to see how small a space is thus left for Captain Maury's land-locked ocean. We have purposely left out of consideration, as yet, the advances made by arctic voyagers in the direction of the sea which lies between Greenland and Spitzbergen. We shall presently see that on this side the imaginary land-locked ocean must be more limited than toward the shores of Asia or America. As it is, however, it remains clear that, if there were any ocean communicating with the spot reached by Dr. Kane, but separated from all communication — by open water — either with the Atlantic or with the Pacific, that ocean would be so limited in extent that the moon's attraction could exert no more effective influence upon its waters than upon the waters of the Mediterranean — where, as we know, no tides are generated. This, then, would be a tideless ocean, and we must look elsewhere for an explanation of the tidal waves seen by Dr. Kane.

We thus seem to have *prima facie* evidence that the sea reached by Kane communicates either with the Pacific or with the Atlantic, or — which is the most probable view — with both those oceans. When we consider the voyages which have been made toward the North Pole along the northerly prolongation of the Atlantic Ocean, we find very strong evidence in favor of the view that there is open-water communication in this direction, not only with the spot reached by Kane, but with a region very much nearer to the North Pole.

So far back as 1607, Hudson had penetrated within 8½° (or about 600 miles) of the North Pole on this route. When we consider the clumsy build and the poor sailing qualities of the ships of Hudson's day, we cannot but feel that so successful a journey marks this route as one of the most promising ever tried. Hudson was not turned back by impassable barriers of land or ice, but by the serious dangers to which the floating masses of ice and the gradually-thickening ice-fields exposed his weak and ill-manned vessel. Since his time, others have sailed upon the same track, and hitherto with no better success. It has been reserved to the Swedish expedition of last year to gain the highest latitudes ever reached in a ship in this direction. The steamship "Sofia," in which this successful voyage was made, was strongly built of Swedish iron, and originally intended for winter voyages in the Baltic. Owing to a number of delays, it was not until September 16th that the "Sofia" reached the most northerly part of her journey. This was a point some fifteen miles nearer the North Pole than Hudson had reached. To the north there still lay broken ice, but packed so thickly that not even a boat could pass through it. So late in the season, it would have been unsafe to wait for a change of weather and a consequent breaking up of the ice. Already the temperature had sunk 16° below the freezing-point; and the enterprising voyagers had no choice but to return. They made, indeed, another push for the north a fortnight later, but only to meet with a fresh repulse. An ice-block with which they came into collision opened a large leak in the vessel's side; and when after great exertions they reached the land, the water already stood two feet over the cabin-floor. In the course of these attempts, the depths of the Atlantic were sounded, and two interesting facts were revealed. The first was that the island of Spitzbergen is connected with Scandinavia by a submarine bank; the second was the circumstance that to the north and west of Spitzbergen the Atlantic is more than two miles deep!

We come now to the most conclusive evidence yet afforded of the extension of the Atlantic Ocean toward the immediate neighborhood of the North Pole. Singularly enough, this evidence is associated not with a sea-voyage, nor with a voyage across ice to the borders of some northern sea, but with a journey during which the voyagers were throughout surrounded as far as the eye could reach by apparently fixed ice-fields.

In 1827 Sir Edward Parry was commissioned by the English Government to attempt to reach the North Pole. A large reward was promised in case he succeeded, or even if he could get within five degrees of the North Pole. The plan which he adopted seemed promising. Starting from a port in Spitzbergen, he proposed to travel as far northward as possible in sea-boats, and then, landing upon the ice, to prosecute his voyage by means of sledges. Few narratives of arctic travel are more interesting than that which Parry has left of this famous "boat-

and-sledge" expedition. The voyagers were terribly harassed by the difficulties of the way; and, after a time, that most trying of all arctic experiences, the bitterly cold wind which comes from out the dreadful north, was added to their trials. Yet still they plodded steadily onward, tracking their way over hundreds of miles of ice with the confident expectation of at least attaining to the eighty-fifth parallel, if not to the Pole itself.

But the most grievous disappointment was in store for them. Parry began to notice that the astronomical observation by which in favorable weather he estimated the amount of their northerly progress, showed a want of correspondence with the actual rate at which they were travelling. At first he could hardly believe that there was not some mistake; but at length the unpleasing conviction was forced upon him that the whole ice-field over which he and his companions had been toiling so painfully was setting steadily southward before the wind. Each day the extent of this set became greater and greater, until at length they were actually carried as far toward the south as they could travel northward.

Parry deemed it useless to continue the struggle. There were certainly two chances in his favor. It was possible that the north wind might cease to blow, and it was also possible that the limit of the ice might soon be reached, and that his boats might travel easily northward upon the open sea beyond. But he had to consider the exhausted state of his men, and the great additional danger to which they were subjected by the movable nature of the ice-fields. If the ice should break up, or if heavy and long-continued southerly winds should blow, they might have found it very difficult to regain their port of refuge in Spitzbergen before winter set in or their stores were exhausted. Besides, there were no signs of water in the direction they had been taking. The water-sky of arctic regions can be recognized by the experienced seaman long before the open sea itself is visible. On every side, however, there were the signs of widely-extended ice-fields. It seemed, therefore, hopeless to persevere, and Parry decided on returning with all possible speed to the haven of refuge prepared for the party in Spitzbergen. He had succeeded in reaching the highest northern latitudes ever yet attained by man.

The most remarkable feature of this expedition, however, is not the high latitude which the party attained, but the strange circumstance which led to their discomfiture. What opinion are we to form of an ocean at once wide and deep enough to float an ice-field which must have been thirty or forty thousand square miles in extent? Parry had travelled upward of three hundred miles across the field, and we may fairly suppose that he might have travelled forty or fifty miles farther without reaching open water; also that the field extended fully fifty miles on each side of Parry's northerly track. That the whole of so enormous a field should have floated freely before the arctic winds is indeed an astonishing circumstance. On every side of this floating ice-island there must have been seas comparatively free from ice; and could a stout ship have forced its way through these seas, the latitudes to which it could have reached would have been far higher than those to which Parry's party was able to attain. For a moment's consideration will show that the part of the great ice-field where Parry was compelled to turn back must have been floating in far higher latitudes when he first set out. He reckoned that he had lost more than a hundred miles through the southerly motion of the ice-field, and by this amount, of course, the point he reached had been nearer the Pole. It is not assuming too much to say that a ship which could have forced its way round the great floating ice-field would certainly have been able to get within four degrees of the Pole. It seems to us highly probable that she would even have been able to sail upon open water to and beyond the Pole itself.

And when we remember the direction in which Dr. Kane saw an open sea — namely, toward the very region where Parry's ice-ship had floated a quarter of a century before — it seems reasonable to conclude that there is open-water communication between the seas which lie to the north of Spitzbergen and those which lave the northwestern shores of Greenland. If this be so, we at once obtain an explanation of the tidal waves which Kane watched day after day in 1855. These had no doubt swept along the valley of the Atlantic, and thence around the northern coast of Greenland. It follows that, densely as the ice may be packed at times in the seas by which Hudson, Scoresby, and other captains, have attempted to reach the North

Pole, the frozen masses must in reality be floating freely, and there must therefore exist channels through which an adventurous seaman might manage to penetrate the dangerous barriers surrounding the polar ocean.

In such an expedition, chance unfortunately plays a large part. Whalers tell us that there is great uncertainty as to the winds which may blow during an arctic summer. The icebergs may be crowded by easterly winds upon the shores of Greenland, or by westerly winds upon the shores of Spitzbergen, or, lastly, the central passage may be the most encumbered, through the effects of winds blowing now from the east and now from the west. Thus the arctic voyager has not merely to take his chance as to the route along which he shall adventure northward, but often, after forcing his way successfully for a considerable distance, he finds the ice-fields suddenly closing in upon him on every side, and threatening to crush his ship into fragments. The irresistible power with which, under such circumstances, the masses of ice bear down upon the stoutest ship has been evidenced again and again; though, fortunately, it not unfrequently happens that some irregularity along one side or the other of the closing channel serves as a sort of natural dock, within which the vessel may remain in comparative safety until a change of wind sets her free. Instances have been known in which a ship has had so narrow an escape in this way, and has been subjected to such an enormous pressure, that when the channel has opened out again, the impress of the ship's side has been seen distinctly marked upon the massive blocks of ice which have pressed against her.

Notwithstanding the dangers and difficulties of the attempt, and the circumstance that no material gains can reward the explorer, it seems not unlikely that before many months are passed the North Pole will have been reached. Last year two bold attempts were made — one by the Swedes, as already mentioned, the other by German men of science. In each case the result was so far successful as to give good promise for future attempts. This year both these nations will renew their attack upon the interesting problem. The German expedition will consist of two vessels, the "Germania" and the "Greenland." The former is a screw-steamer, of 126 tons, and well adapted to encounter the buffets of the ice-masses which are borne upon the arctic seas. The other is a sailing-yacht of 80 tons, and is intended to act as a transport-ship, by means of which communication may be kept up with Europe. The "Germania" will probably winter in high northern latitudes; and we should not be much surprised if before her return she should have been carried to the very Pole. Nor can the prospects of the Swedish expedition be considered less promising, when we remember that last year, though hampered by the lateness of the season and other difficulties, they succeeded in approaching the Pole within a distance only a few miles greater than that which separated Parry from the Pole in 1829.

Certainly England has reason to fear that before the year 1870 has closed she will no longer be able to claim that her flag has approached both Poles more nearly than the flag of any other nation. There are considerations which make the recent supineness of our country in the matter of arctic travel much to be regretted. In the winter of 1874 there will occur one of those interesting phenomena by which Nature occasionally teaches men useful lessons respecting her economy. We refer to the transit of Venus on December 8th in that year. One of the most effective modes of observing this transit will require that a party of scientific men should penetrate far within the recesses of the desolate Antarctic Circle. Where are the trained arctic seamen to be found who will venture upon this service? Most of our noted arctic voyagers have earned their rest; and, as Commander Davis said at a recent meeting of the Geographical Society, those who go for the first time into the arctic or antarctic solitudes are too much tried by the effects of the new experience to be fit to undertake important scientific labors. He spoke with special reference to the transit of 1882, for the observation of which there is (I have lately shown) small occasion to employ arctic voyagers. It is just possible that for the transit of 1874 trained explorers belonging to the old school of arctic travel may still be found. But if not, no time should be lost in supplying the deficiency. I have shown within the last few months that journeys to the antarctic regions will be required for this transit, and not for the later transit (as had been supposed). The Astronomer-Royal has expressed his desire that the discovery may be rendered available by suitable expeditions. "Every series of observations," he remarks, "which can really be brought to bear upon this important determination will be valuable." Therefore, for this reason alone, and even if the reputation of England in the matter of arctic travel were altogether worthless, it would be well that efforts should quickly be made to prepare crews and commanders for the work of 1874, by "sending them to school," as Commander Davis expressed it, "in the arctic seas."

IS THE GULF STREAM A MYTH?

The Gulf Stream has recently attracted a large share of the attention of our men of science. The abnormal character of the weather which we experienced last winter has had something to do with this. The influence of the Gulf Stream upon our climate, and the special influence which it is assumed to exercise in mitigating the severity of our winters, have been so long recognized that meteorologists began to inquire what changes could be supposed to have taken place in the great current to account for so remarkable a winter as the last. But it happened also that at a meeting of the Royal Geographical Society early in the present year the very existence of the Gulf Stream was called in question, just when meteorologists were disposed to assign to it effects of unusual importance. And in the course of the discussion whether there is in truth a Gulf Stream — or rather whether our shores are visited by a current which merits such a name — a variety of interesting facts were adduced, which were either before unknown or had attracted little attention. As at a recent meeting of the same society these doubts have been renewed, we propose to examine briefly, in the first place, a few of the considerations which have been urged against the existence of a current from the Gulf of Mexico to the neighborhood of our shores; and then, having rehabilitated the reputation of this celebrated ocean-river — as we believe we shall be able to do — we shall proceed to give a brief sketch of the processes by which the current-system of the North Atlantic is set and maintained in motion.

In reality the Gulf Stream is only a part of a system of oceanic circulation; but in dealing with the arguments which have been urged against its very existence, we may confine our attention to the fact that, according to the views which had been accepted for more than a century, there is a stream of water which, running out of the Gulf Stream through the Narrows of Bemini, flows along the shores of the United States to Newfoundland, and thence right across the Atlantic to the shores of Great Britain. It is this last fact which is now called in question. The existence of a current as far as the neighborhood of Newfoundland is conceded, but the fact that the stream flows onward to our shores is denied.

The point on which the most stress is placed is the shallowness of the passage called the "Bemini Narrows," through which it is assumed that the whole of the Gulf current must pass. This passage has a width of about forty miles, and a depth of a little more than six hundred yards. The current which flows through it is perhaps little more than thirty miles in width, and a quarter of a mile in depth. It is asked with some appearance of reason, how this narrow current can be looked upon as the parent of that wide stream which is supposed to traverse the Atlantic with a mean width of some five or six hundred miles. Indeed, a much greater width has been assigned to it, though on mistaken grounds; for it has been remarked that since waifs and strays from the tropics are found upon the shores of Portugal, as well as upon those of Greenland, we must ascribe to the current a span equal to the enormous space separating these places. But the circumstance here dwelt upon can clearly be explained in another way. We know that of two pieces of wood thrown into the Thames at Richmond, one might be picked up at Putney, and the other at Gravesend. Yet we do not conclude that the width of the Thames is equal to the distance separating Putney from Gravesend. And doubtless the tropical waifs which have been picked up on the shores of Greenland and of Portugal have found their way thither by circuitous courses, and not by direct transmission along opposite edges of the great Gulf current.

But certainly the difficulty associated with the narrowness of the Bemini current is one deserving of careful attention. Are we free to identify a current six hundred miles in width with one which is but thirty miles wide, and not very deep? An increase of width certainly not less than thirtyfold would appear to correspond to a proportionate diminution of depth. And remembering that it is only near the middle of the Narrows that the Gulf Stream has a depth of four hundred yards, we could scarcely assign to the wide current in the mid-Atlantic a greater depth than ten or twelve yards. This depth seems altogether out of proportion to the enormous lateral extension of the current.

But besides that even this consideration would not suffice to disprove the existence of a current in the mid-Atlantic, an important circumstance remains to be mentioned. The current in the Narrows flows with great velocity — certainly not less than four or five miles an hour. As the current grows wider it flows more sedately; and opposite Cape Hatteras its velocity is already reduced to a little more than three miles an hour. In the mid-Atlantic the current may be assumed to flow at a rate little exceeding a mile per hour, at the outside. Here, then, we have a circumstance which suffices to remove a large part of the difficulty arising from the narrowness of the Bemini current, and we can at once increase our estimate of the depth of the mid-Atlantic current fivefold.

But this is not all. It has long been understood that the current which passes out through the Narrows of Bemini corresponds to the portion of the great equatorial current which passes into the Gulf of Mexico between the West-Indian Islands. We cannot doubt that the barrier formed by those islands serves to divert a large portion of the equatorial current. The portion thus diverted finds its way, we may assume, along the outside of the West-Indian Archipelago, and thus joins the other portion — which has in the meantime made the circuit of the Gulf — as it issues from the Bemini Straits. All the maps in which the Atlantic currents are depicted present precisely such an outside current as we have here spoken of, and most of them assign to it a width exceeding that of the Bemini current. Indeed, were it not for the doubts which the recent discussions have thrown upon all the currents charted by seamen, we should have been content to point to this outside current as shown in the maps. As it is, we have thought it necessary to show that such a current must necessarily have an existence, since we cannot lose sight of the influence of the West-Indian Isles in partially damming up the passage along which the equatorial current would otherwise find its way into the Gulf of Mexico. Whatever portion of the great current is thus diverted must find a passage elsewhere, and no passage exists for it save along the outside of the West-Indian Isles.

The possibility that the wide current which has been assumed to traverse the mid-Atlantic may be associated with the waters which flow from the Gulf of Mexico, either through the Narrows or round the outside of the barrier formed by the West Indies, has thus been satisfactorily established. But we now have to consider difficulties which have been supposed to encounter our current on its passage from the Gulf to the mid-Atlantic.

Northward, along the shores of the United States, the current has been traced by the singular blueness of its waters until it has reached the neighborhood of Newfoundland. Over a part of this course, indeed, the waters of the current are of indigo blue, and so clearly marked that their line of junction with the ordinary sea-water can be traced by the eye. "Often," says Captain Maury, "one half of a vessel may be perceived floating in Gulf-Stream water, while the other half is in common water of the sea — so sharp is the line, and such the want of affinity between the waters, and such, too, the reluctance, so to speak, on the part of those of the Gulf Stream to mingle with the littoral waters of the sea."

But it is now denied that there is any current beyond the neighborhood of Newfoundland — or that the warm temperature, which has characterized the waters of the current up to this point, can be detected farther out.

It is first noticed that, as the Gulf current must reach the neighborhood of Newfoundland with a northeasterly motion, and, if it ever reached the shores of the British Isles, would have to travel thither with an almost due easterly motion, there is a change of direction to be accounted for. This, however, is an old, and we had supposed exploded, fallacy. The course of the Gulf Stream from the Bemini Straits to the British Isles corresponds exactly with that which is due to the combined effects of the motion of the water and that of the earth upon its axis. Florida being much nearer than Ireland to the equator, has a much more rapid easterly motion. Therefore, as the current gets farther and farther north, the effect of the easterly motion thus imparted to it begins to show itself more and more, until the current is gradually changed from a northeasterly to an almost easterly stream. The process is the exact converse of that by which the air-currents from the north gradually change into the northwesterly trade-winds as they get farther south.

But it is further remarked that as the current passes out beyond the shelter of Newfoundland, it is impinged upon by those cold currents from the arctic seas which are known to be continually flowing out of Baffin's Bay and down the eastern shores of Greenland; and it is contended that these currents suffice, not merely to break up the Gulf current, but so to cool its waters that these could produce no effect upon the climate of Great Britain if they ever reached its neighborhood.

Here, again, we must remark that we are dealing with no new discovery. Captain Maury has already remarked upon this peculiarity. "At the very season of the year," he says, "when the Gulf Stream is rushing in greatest volume through the Straits of Florida, and hastening to the north with the greatest rapidity, there is a cold stream from Baffin's Bay, Labrador, and the coasts of the north, running south with equal velocity. . . . One part of it underruns the Gulf Stream, as is shown by the icebergs, which are carried in a direction tending across its course." There can be no doubt, in fact, that this last circumstance indicates the manner in which the main contest between the two currents is settled. A portion of the arctic current finds its way between the Gulf Stream and the continent of America; and this portion, though narrow, has a very remarkable effect in increasing the coldness of the American winters. But the main part, heavier, by reason of its coldness, than the surrounding water, sinks beneath the surface. And the well-known fact mentioned by Maury, that icebergs have been seen stemming the Gulf Stream, suffices to show how comparatively shallow that current is at this distance from its source, and thus aids to remove a difficulty which we have already had occasion to deal with.

Doubtless the cooling influence of the arctic currents is appreciable; but it would be a mistake to suppose that this influence can suffice to deprive the Gulf current of its distinctive warmth. If all the effect of the cold current were operative on the Gulf Stream alone, we might suppose that, despite the enormous quantity of comparatively warm water which is continually being carried northward, the current would be reduced to the temperature of the surrounding water. But this is not so. The arctic current not only cools the Gulf current, but the surrounding water also — possibly to a greater extent, for it is commonly supposed that a bed of ordinary sea-water separates the two main currents from each other. Thus the characteristic difference of temperature remains unaffected. But in reality we may assume that the cooling effect actually exercised by the arctic current upon the neighboring sea is altogether disproportionate to the immense amount of heat continually being carried northward by the Gulf Stream. It is astonishing how unreadily two sea-currents exchange their temperatures — to use a somewhat inexact mode of expression. The very fact that the littoral current of the United States is so cold — a fact thoroughly established — shows how little warmth this current has drawn from the neighboring seas. Another fact, mentioned by Captain Maury, bears in a very interesting manner upon this peculiarity. He says: "If any vessel will take up her position a little to the northward of Bermuda, and steering thence for the capes of Virginia, will try the water-thermometer all the way at short intervals, she will find its reading to be now higher, now lower; and the observer will discover that he has been crossing streak after streak of warm and cool water in regular alternations." Each portion maintains its own temperature even in the case of such warm streaks as these, all belonging to one current.

Similar considerations dispose of the arguments which have been founded on the temperature of the sea-bottom. It has been proved that the living creatures which people the lower depths of the sea exist under circumstances which evidence a perfect uniformity of temperature; and arguments on the subject of the Gulf Stream have been derived from the evidence of what is termed a minimum thermometer — that is, a thermometer which will indicate the lowest temperature it has been exposed to — let down into the depths of the sea. All such arguments, whether adduced against or in favor of the Gulf-Stream theory, must be held to be futile, since the thermometer in its descent may pass through several submarine currents of different temperature.

Lastly, an argument has been urged against the warming effects of the Gulf Stream upon our climate, which requires to be considered with some attention. It is urged that the warmth derived from so shallow a current as the Gulf Stream must be, by the time it has reached our shores, could not provide an amount of heat sufficient to affect our climate to any appreciable extent. The mere neighborhood of this water at a temperature slightly higher than that due to the latitude could not, it is urged, affect the temperature of the inland counties at all.

This argument is founded on a misapprehension of the beautiful arrangement by which Nature carries heat from one region to distribute it over another. Over the surface of the whole current the process of evaporation is going on at a greater rate than over the neighboring seas, because the waters of the current are warmer than those which surround them. The vapor thus rising above the Gulf Stream is presently wafted by the southwesterly winds to our shores and over our whole land. But as it thus reaches a region of comparative cold the vapor is condensed — that is, turned into fog, or mist, or cloud, according to circumstances. It is during this change that it gives out the heat it has brought with it from the Gulf Stream. For precisely as the evaporation of water is a process requiring heat, the change of vapor into water — whether in the form of fog, mist, cloud, or rain — is a process in which heat is given out. Thus it is that the southwesterly wind, the commonest wind we have, brings clouds and fogs and rain to us from the Gulf Stream, and with them brings the Gulf-Stream warmth.

Why the southwesterly winds should be so common, and how it is that over the Gulf Stream there is a sort of air-channel along which winds come to us as if by their natural pathway, we have not space here to inquire. The subject is full of interest, but it does not belong to the question we are considering.

It would seem that a mechanism involving the motion of such enormous masses of water as the current-system of the Atlantic should depend on the operation of very evident laws. Yet a variety of contradictory hypotheses have been put forward from time to time respecting this system of circulation, and even now the scientific world is divided between two opposing theories.

Of old the Mississippi River was supposed to be the parent of the Gulf Stream. It was noticed that the current flows at about the same rate as the Mississippi, and this fact was considered sufficient to support the strange theory that a river can give birth to an ocean-current.

It was easy, however, to overthrow this theory. Captain Livingston showed that the volume of water which is poured out of the Gulf of Mexico in the form of an ocean-stream is more than a thousand times greater than the volume poured into the Gulf by the Mississippi River.

Having overthrown this old theory of the Gulf Stream, Captain Livingston attempted to set up one which is equally unfounded. He ascribed the current to the sun's apparent yearly motion and the influence he exerts on the waters of the Atlantic. A sort of yearly tide is conceived, according to this theory, to be the true parent of the Gulf current. It need hardly be said, however, that a phenomenon which remains without change through the winter and summer seasons cannot possibly be referred to the operation of such a cause as a yearly tide.

It is to Dr. Franklin that we owe the first theory of the Gulf Stream which has met with general acceptance. He held that the Gulf Stream is formed by the outflow of waters which have been forced into the Caribbean Sea by the trade-winds; so that the pressure of these winds on the Atlantic Ocean forms, according to Dr. Franklin, the true motive power of the Gulf-Stream machinery. According to Maury, this theory has "come to be the most generally-received opinion in the mind of sea-faring people." It supplies a moving force of undoubted efficiency. We know that, as the trade-winds travel toward the equator, they lose their westerly motion. It is reasonable to suppose that this is caused by friction against the surface of the ocean, to which, therefore, a corresponding westerly motion must have been imparted.

There is a simplicity about Franklin's theory which commends it favorably to our consideration. But when we examine it somewhat more closely, several very decided flaws present themselves to our attention.

Consider, in the first place, the enormous mass of water moved by the supposed agency of the winds. Air has a weight — volume for volume — which is less than one eight-hundredth part of that of water. So that, to create a water-current, an air-current more than eight hundred times as large and of equal velocity must expend the whole of its motion. Now, the trade-winds are gentle winds, their velocity scarcely exceeding in general that of the more swiftly-moving portions of the Gulf Stream. But even assigning to them a velocity four times as great, we still want an air-current two hundred times as large as the water-current. And the former must give up the whole of its motion, which, in the case of so elastic a substance as air, would hardly happen, the upper air being unlikely to be much affected by the motion of the lower.

But this is far from being all. If the trade-winds blew throughout the year, we might be disposed to recognize their influence upon the Gulf Stream as a paramount, if not the sole one. But this is not the case. Captain Maury states that, "with the view of ascertaining the average number of days during the year that the northeast trade-winds of the Atlantic operate upon the currents between 25° north latitude and the equator, log-books containing no less than 380,284 observations on the force and direction of the wind in that ocean were examined. The data thus afforded were carefully compared and discussed. The results show that within these latitudes — and on the average — the wind from the northeast is in excess of the winds from the southwest only 111 days out of the 365. Now, can the northeast trades," he pertinently asks, "by blowing for less than one-third of the time, cause the Gulf Stream to run all the time, and without varying its velocity either to their force or to their prevalence?"

And besides this, we have to consider that no part of the Gulf Stream flows strictly before the trade-winds. Where the current flows most rapidly, namely, in the Narrows of Bemini, it sets against the wind, and for hundreds of miles after it enters the Atlantic, "it runs," says Maury, "right in the 'wind's eye.'" It must be remembered that a current of air directed with considerable force against the surface of still water has not the power of generating a current which can force its way far through the resisting fluid. If this were so, we might understand how the current, originating in sub-tropical regions, could force its way onward after the moving force had ceased to act upon it, and even carry the waters of the current right against the wind, after leaving the Gulf of Mexico. But experience is wholly opposed to this view. The most energetic currents are quickly dispersed when they reach a wide expanse of still water. For example, the Niagara below the falls is an immense and rapid river. Yet when it reaches Lake Ontario, " instead of preserving its character as a distinct and well-defined stream for several hundred miles, it spreads itself out, and its waters are immediately lost in those of the lake." Here, again, the question asked by Maury bears pertinently on the subject we are considering. " Why," he says, " should not the Gulf Stream do the same? It gradually enlarges itself, it is true; but, instead of mingling with the ocean by broad spreading, as the immense rivers descending into the northern lakes do, its waters, like a stream of oil in the ocean, preserve a distinctive character for more than three thousand miles."

The only other theory which has been considered in recent times to account satisfactorily for all the features of the Gulf-Stream mechanism was put forward, we believe, by Captain Maury. In this theory, the motive power of the whole system of oceanic circulation is held to be the action of the sun's heat upon the waters of the sea. We recognize two contrary effects as the immediate results of the sun's action. In the first place, by warming the equatorial waters, it tends to make them lighter; in the second place, by causing evaporation, it renders them salter, and so tends to make them heavier. We have to inquire which form of action is most effective. The inquiry would be somewhat difficult, if we had not the evidence of the sea itself to supply an answer. For it is an inquiry to which ordinary experimental processes would not be applicable. We must accept the fact that the heated water from the equatorial seas actually does float upon the cooler portions of the Atlantic, as evidence that the action of the sun results in making the water lighter.

Now, Maury says that the water thus lightened must flow over and form a surface-current toward the Poles; while the cold and heavy water from the polar seas, as soon as it reaches the temperate zone, must sink and form a submarine current. He recognizes in these facts the mainspring of the whole system of oceanic circulation. If a long trough be divided into two compartments, and we fill one with oil and the other with water, and then remove the dividing plate, we shall see the oil rushing over the water at one end of the trough, and the water rushing under the oil at the other. And if we further conceive that oil is continually being added at that end of the trough originally filled with oil, while water is continually added to the other, it is clear that the system of currents would continue in action: that is, there would be a continual flow of oil in one direction along the surface of the water, and of water in the contrary direction underneath the oil.

But Sir John Herschel maintains that no such effects as Maury describes could follow the action of the sun's heat upon the equatorial waters. He argues thus: Granting that these waters become lighter and expand in volume, yet they can only move upward, downward, or sideways. There can be nothing to cause either of the first two forms of motion; and as for motion sideways, it can only result from the gradual slope caused by the bulging of the equatorial waters. He proceeds to show that this slope is so slight that we cannot look upon it as competent to form any sensible current from the equatorial toward the polar seas. And even if it could, he says, the water thus flowing off would have an eastward instead of a westward motion, precisely as the counter-trade-winds, blowing from equatorial to polar regions, have an eastward motion.

It is singular how completely the supporter of each rival view has succeeded in overthrowing the arguments of his opponent. Certainly Maury has shown with complete success that the inconstant trade-winds cannot account for the constant Gulf current, which does not even flow before them, but, in places, exactly against their force. And the reasoning of Sir John Herschel seems equally cogent, for certainly the flow of water from equatorial toward polar regions ought from the first to have an eastward, instead of a westward motion; whereas the equatorial current, of which the Gulf Stream is but the continuation, flows from east to west, right across the Atlantic.

Equally strange is it to find that each of these eminent men, having read the arguments of the other, reasserts, but does not effectually defend, his own theory, and repeats with even more damaging effect his arguments against the rival view.

Yet one or other theory must at least point to the true view, for the Atlantic is subject to no other agencies which can for a moment be held to account for a phenomenon of such magnificence as the Gulf Stream.

It appears to us that, on a close examination of the Gulf-Stream mechanism, the true mainspring of its motion can be recognized. Compelled to reject the theory that the trade-winds generate the equatorial current westward, let us consider whether Herschel's arguments against the "heat-theory" may not suggest a hint for our guidance. He points out that an overflow from the equator poleward would result in an eastward, and not in a westward current. This is true. It is equally true that a flow of water toward the equator would result in a westward current. But no such a flow is observed. Is it possible that there may be such a flow, but that it takes place in a hidden manner? Clearly there may be. Sub-marine currents toward the equator would have precisely the kind of motion we require, and, if any cause drew them to the surface near the equator, they would account in full for the great equatorial westward current.

At this point we begin to see that an important circumstance has been lost sight of in dealing with the heat-theory. The action of the sun on the surface-water of the equatorial Atlantic has only been considered with reference to its warming effects. But we must not forget that this action has drying effects also. It evaporates enormous quantities of water, and we have to inquire whence the water comes by which the sea-level is maintained. A surface-flow from the sub-tropical seas would suffice for this purpose, but no such flow is observed. Whence, then, can the water come but from below? Thus we recognize the fact that a process resembling suction is continually taking place over the whole area of the equatorial Atlantic, the agent being the intense heat of the tropical sun. No one can doubt that this agent is one of adequate power. Indeed, the winds, conceived by Franklin to be the primary cause of the Atlantic currents, are in reality due to the merest fraction of the energy inherent in the sun's heat.

We have other evidence that the indraught is from below, in the comparative coldness of the equatorial current. The Gulf Stream is warm by comparison with the surrounding waters, but the equatorial current is cooler than the tropical seas. According to Professor Ansted, the southern portion of the equatorial current, as it flows past Brazil, "is everywhere a cold current, generally from four to six degrees below the adjacent ocean."

Having once detected the mainspring of the Gulf-Stream mechanism, or rather of the whole system of oceanic circulation — for the movements observed in the Atlantic have their exact counterpart in the Pacific — we have no difficulty in accounting for all the motions which that mechanism exhibits. We need no longer look upon the Gulf Stream as the rebound of the equatorial current from the shores of North America. Knowing that there is an underflow toward the equator, we see that there must be a surface-flow toward the Poles. And this flow must as inevitably result in an easterly motion, as the underflow toward the equator results in a westerly motion. We have, indeed, the phenomena of the trades and counter-trades exhibited in water-currents instead of air-currents.

FLOODS IN SWITZERLAND.

During the past few weeks we have witnessed a succession of remarkable evidences of Nature's destructive powers. The fires of Vesuvius, the earth-throes of the sub-equatorial Andes, and the submarine disturbance which has shaken Hawaii, have presented to us the various forms of destructive action which the earth's subterranean forces can assume. In the disastrous floods which have recently visited the Alpine cantons of Switzerland, we have evidence of the fact that natural forces which we are in the habit of regarding as beneficent and restorative may exhibit themselves as agents of the most wide-spread destruction. We point out elsewhere in these essays how enormous is the amount of power of which the rain-cloud is the representative; and in doing so we have endeavored to exhibit the contrast between the steady action of the falling shower and the energy of the processes of which rain is in reality the equivalent. But in the floods which have lately ravaged Switzerland we see the same facts illustrated, not by numerical calculations or by the results of philosophical experiments, but in action, and that action taking place on the most widely-extended scale. The whole of the southeastern, or, as it may be termed, the Alpine half of Switzerland, has suffered from these floods. If a line be drawn from the Lake of Constance, in the northeast of Switzerland, to the Col de Balme, in the southwest, it will divide Switzerland into two nearly equal portions, and scarcely a canton within the eastern of these divisions has escaped without great damage.

The cantons which have suffered most terribly are those of Tessin, Grisons, and St. Gall. The St. Gothard, Splugen, and St. Bernhardin routes, have been rendered impassable. Twenty-seven lives were lost in the St. Gothard Pass, besides horses and wagons full of merchandise. It is stated that on the three routes upward of eighty persons perished. In the village of Loderio alone, no less than fifty deaths occurred. So terrible a flood has not taken place since the year 1834. Nor have the cantons of Uri and Valais escaped. From Unterwalden we hear that the heavy rains which took place a fortnight ago have carried away several large bridges, and many of the rivers continue still very swollen. We have already described how enormous the material losses are which have been caused by these floods. Many places are under water; others in ruins or absolutely destroyed. In Tessin alone the damage is estimated at forty thousand pounds sterling.

A country like Switzerland must always be liable to the occurrence, from time to time, of catastrophes of this sort. Or rather, perhaps, we should draw a distinction between the two divisions of Switzerland referred to above. Of these the one may be termed the mountain half, and the other the lake half of the country. It is the former portion of the country which is principally subject to the dynamical action of water. A long-continued and heavy rainfall over the higher lands cannot fail to produce a variety of remarkable effects, where the arrangement of mountains and passes, hills, valleys, and ravines, is so complicated. There are places where a large volume of water can accumulate until the barriers which have opposed its passage to the plains burst under its increasing weight; and then follow those destructive rushes of water which sweep away whole villages at once. It is, in fact, the capacity of the Swiss mountain-region for damming up water, far more than any other circumstance, which renders the Swiss floods so destructive.

And then it must be remembered that there are at all times suspended over the plains and valleys which lie beneath the Alpine ranges enormous masses of water in the form of snow and ice. Although in general these suffer no changes but those due to the partial melting which takes place in summer, and the renewed accumulation which takes place in winter, yet when heavy rains fall upon the less elevated portions of the Alpine snow, they not only melt that snow much more rapidly than the summer sun would do, but they wash down large masses, which add largely to the destructive power of the descending waters.

The most destructive floods which have occurred in Switzerland have usually been those which take place in early summer. The floods which inundated the plains of Martigny in 1818 were a remarkable instance of the effects which result from the natural damming up of large volumes of water in the upper parts of the Alpine hill-country. The whole of the valley of Bagnes, one of the largest of the lateral branches of the main valley of the Rhone above Geneva, was converted into a lake, in the spring of 1818, by the damming up of a narrow pass into which avalanches of snow and ice had been precipitated from a lofty glacier overhanging the bed of the river Dranse. The icy barrier enclosed a lake no less than half a league in length and an eighth of a mile wide, and in places two hundred feet deep. The inhabitants of the neighboring villages were terrified by the danger which was to be apprehended from the bursting of the barrier. They cut a gallery seven hundred feet long through the ice, while the waters had as yet risen to but a moderate height; and when the waters began to flow through this channel, its course was deepened by the melting of the ice, and at length nearly half the contents of the lake were safely carried off. It was hoped that the process would continue, and the country be saved from the danger which had been so long impending over it. But as the heat of the weather increased, the central part of the barrier slowly melted away, until it became too weak to bear the enormous weight of water which was pressing against it. At length it gave way, so suddenly and completely that all the water which remained in the lake rushed out in half an hour. The downward passage of the water illustrated, in a very remarkable way, the fact that the chief mischief of floods is occasioned where water is checked in its outflow. For it is related that, "in the course of their descent the waters encountered several narrow gorges, and at each of these they rose to a great height, and then burst with new violence into the next basin, sweeping along forests, houses, bridges, and cultivated land." Along the greater part of its course the flood resembled rather a moving mass of rock and mud than a stream of water. Enormous masses of granite were torn out of the sides of the valleys and whirled for hundreds of yards along the course of the flood. M. Escher relates that one of the fragments thus swept along was no less than sixty yards in circumference. At first the water rushed onward at a rate of more than a mile in three minutes, and the whole distance (forty-five miles) which separates the valley of Bagnes from the Lake of Geneva was traversed in little more than six hours. The bodies of persons who had been drowned in Martigny were found floating on the farther side of the Lake of Geneva, near Vevey. Thousands of trees were torn up by the roots, and the ruins of buildings which had been overthrown by the flood were carried down beyond Martigny. In fact, the flood at this point was so high that some of the houses in Martigny "were filled with mud up to the second story." Beyond Martigny the flood did but little damage, as it here expanded over the plain, and was at once reduced in depth and velocity.

A GREAT TIDAL WAVE.

DURING the last few days anxious questionings have been heard respecting the next spring tides. A certain naval officer, who conceives that he can trace in the relative positions of the sun and moon the secret of every important change of weather, has described in the columns of a contemporary the threatening significance of the approaching conjunction of the sun and moon. He predicts violent atmospheric disturbances; though in another place he tells us merely that the conjunction is to cause "unsettled weather," a state of matters to which we in England have become tolerably well accustomed.

But people are asking what is the actual relation which is to bring about such terrible events. The matter is very simple. On October 5th, the moon will be new — in other words, if it were not for the brightness of the sun, we should see the moon close by that luminary on the heavens. Thus the sun and moon will pull with combined effect upon the waters of the earth, and so cause what are called spring tides. This, of course, happens at the time of every new moon. But sometimes the moon exerts a more effective pull than at other times; and the same happens also in the case of the sun; and on October 5th, it happens that both the sun and the moon will give a particularly vigorous haul upon the earth's waters. As regards the sun, there is nothing unusual. Every October his pull on the ocean is much the same as in preceding Octobers. But October is a month of high solar tides — and for these reasons: In September, as every one knows, the sun crosses the equinoctial; and other things being equal it would be when on the equinoctial that his power to raise a tidal wave would be greatest. But other things are not equal; for the sun is not always at a fixed distance from the earth. He is nearest in January; so that he would exert more power in that month than in any other if his force depended solely on distance. As matters actually stand, it will be obvious that at some time between September and January the sun's tidal power would have a maximum value. Thus it is that October is a month of high solar tidal waves.

But is it the lunar wave which will be most effectively strengthened at the next spring tide. If we could watch the lunar tidal wave alone (instead of always finding it combined with the solar wave) we should find it gradually increasing, and then gradually diminishing, in a period of about a lunar month. And we should find that it was always largest when the moon looked largest, and *vice versa.* In other words, when the moon is in perigee the lunar wave is largest. But then there is another consideration. The lunar wave would vary according to the moon's proximity to the equinoctial; and (other things being equal) would be largest when the moon is exactly opposite the earth's equator. If the two effects are combined, that is, if the moon happens to be in perigee and on the equinoctial at the same time, then of course we get the largest lunar tidal wave we can possibly have.

Now, this "largest lunar wave" occurs at somewhat long intervals, because the relation on which it depends is one which is, so to speak, exceptional. Still the relation does recur, and with a certain degree of regularity. When it happens, however, it by no means follows that we have a very high tide; because it may occur when the tides are near "neap"; in other words, when the sun and moon exert opposing effects. The largest lunar wave cannot stand the drain which the solar wave exerts upon it at the time of neap tides. Nor would the large lunar tidal wave produce an exceptionally high tide, even though it were not the time of "neap," or were tolerably near the time of "spring" tides. Only when it happens that a large lunar wave combines fully with the solar wave do we get very high tides. And when, in addition to this relation, we have the solar wave nearly at a maximum, we get the highest of all possible tides. This is what will happen, or all but happen, on October 5th next. The combination of circumstances is almost the most effective that can possibly exist.

But, after all, high tides depend very importantly on other considerations than astronomical ones. Most of us remember how a predicted high tide some two years ago turned out to be very moderate, or, if we may use the expression, a very "one-horse" affair indeed, because the winds had not been consulted, and exerted their influence against the astronomers. A long succession of winds blowing off-shore would reduce a spring tide to a height scarcely exceeding the ordinary neap. On the other hand, if we should have a long succession of westerly winds from the Atlantic before the approaching high tide, it is certain that a large amount of mischief may be done in some of our river-side regions.*

As for the predicted weather changes, they may be regarded as mere moonshine. A number of predictions, founded on the motions of the sun and moon, have found a place during many months past in the columns of a contemporary; but there has been no greater agreement between these predictions and the weather actually experienced than any one could trace between Old More's weather prophecies and recorded weather changes. In other words, there have been certain accordances which would be very remarkable indeed if they did not happen to be associated with as many equally remarkable discordances. Random predictions would be quite as satisfactory.

A very amusing misprint has found its way into many newspapers in connection with the coming tide. It is interesting as serving to show how little is really known by the general public about some of the simplest scientific matters. The original statement announced that the sun would not be in perihelion by so many seconds of semi-diameter, in itself a very incorrect mode of expression. Still it was clear that what was meant was, that the earth would be so far from the place of nearest approach to the sun that the latter would not look as large as it possibly can by so many seconds of semi-diameter. In many papers, however, we read that the "sun will not be in perihelion by so many seconds of mean chronometer"! Who first devised this marvellous reading is unknown — he should have a statue.

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* The wave did little mischief.

DEEP-SEA DREDGINGS.

MEN have ever been strangely charmed by the unknown and the seemingly inaccessible. The astronomer exhibits the influence of this charm as he constructs larger and larger telescopes, that he may penetrate more and more deeply beyond the veil which conceals the greater part of the universe from the unaided eye. The geologist seeking to piece together the fragmentary records of the past which the earth's surface presents to him, is equally influenced by the charm of mystery and difficulty. And the microscopist who tries to force from Nature the secret of the infinitely little, is led on by the same strange desire to discover just those matters which Nature has been most careful to conceal from us.

The energy with which in recent times men have sought to master the problem of deep-sea sounding and deep-sea dredging is, perhaps, one of the most striking instances ever afforded of the charm which the unknown possesses for mankind. Not long ago, one of the most eminent geographers of the sea spoke regretfully about the small knowledge men have obtained of the depths of ocean. "Greater difficulties," he remarked, "than any presented by the problem of deep-sea research have been overcome in other branches of physical inquiry. Astronomers have measured the volumes and weighed the masses of the most distant planets, and increased thereby the stock of human knowledge. Is it creditable to the age that the depths of the sea should remain in the category of unsolved problems? that its 'ooze and bottom' should be a sealed volume, rich with ancient and eloquent legends, and suggestive of many an instructive lesson that might be useful and profitable to man?"

Since that time, however, deep-sea dredging has gradually become more and more thoroughly understood and mastered. Recently, when the telegraphic cable which had lain so many months at the bottom of the Atlantic was hauled on board the "Great Eastern" from enormous depths, men were surprised and almost startled by the narrative. The appearance of the ooze-covered cable as it was slowly raised toward the surface, and the strange thrill which ran through those who saw it and remembered through what mysterious depths it had twice passed; its breaking away almost from the very hands of those who sought to draw it on board; and the successful renewal of the attempt to recover the cable — all these things were heard of as one listens to a half-incredible tale. Yet when that work was accomplished deep-sea dredging had already been some time a science, and many things had been achieved by its professors which presented, in reality, greater practical difficulties than the recovery of the Atlantic Cable.

Recently, however, deep-sea researches have been carried on with results which are even more sensational, so to speak, than the grappling feat which so surprised us. Seas so deep that many of the loftiest summits of the Alps might be completely buried beneath them have been explored. Dredges weighing with their load of mud nearly half a ton have been hauled up without a hitch from depths of some 14,000 feet. But not merely has comparatively rough work of this sort been achieved, but by a variety of ingenious contrivances men of science have been able to measure the temperature of the sea at depths where the pressure is so enormous as to be equivalent to a weight of more than 430 tons on every square foot of surface.

The results of these researches are even more remarkable and surprising, however, than the means by which they have been obtained. Sir Charles Lyell has fairly spoken of them as so astonishing "that they have to the geologist almost a revolutionary character." Let us consider a few of them.

No light can be supposed to penetrate to the enormous depths just spoken of. Therefore, how certainly we might conclude that there can be no life there! If, instead of dealing with the habitability of planets, Whewell, in his "Plurality of Worlds," had been considering the question whether at depths of two or three miles living creatures could subsist, how convincingly would he have proved the absurdity of such a supposition! Intense cold, perfect darkness, and a persistent pressure of two or three tons to the square inch — such, he might have argued, are the conditions under which life exists, if at all, in those dismal depths. And even if he had been disposed to concede the bare possibility that life of some sort may be found there, then certainly he would have urged, some new sense must replace sight — the creatures in these depths can assuredly have no eyes, or only rudimentary ones.

But the recent deep sea-dredgings have proved that not only does life exist in the very deepest parts of the Atlantic, but that the beings which live and move and have their being beneath the three-mile mountain of water have eyes which the ablest naturalists pronounce to be perfectly developed. Light, then, of some sort must exist in those abysms, though whether the home of the deep-sea animals be phosphorescent, as Sir Charles Lyell suggests, or how light may reach these creatures, we have no present means of determining.

If there is one theory which geologists have thought more justly founded than all others, it is the view that the various strata of the earth were formed at different times. A chalk district, for example, lying side by side with a sandstone district, has been referred to a totally different era. Whether the chalk was formed first, or whether the sandstone existed before the minute races came into being which formed the cretaceous stratum, might be a question. But no doubt existed in the minds of geologists that each formation belonged to a distinct period. Now, however, Dr. Carpenter and Professor Thomson may fairly say, "We have changed all this." It has been found that at points of the sea-bottom only eight or ten miles apart, there may be in progress the formation of a cretaceous deposit and of a sandstone region, each with its own proper fauna. "Wherever similar conditions are found upon the dry land of the present day," remarks Dr. Carpenter, "it has been supposed that the formation of chalk and the formation of sandstone must have been separated from each other by long periods, and the discovery that they may actually coexist upon adjacent surfaces has done no less than strike at the very root of the customary assumptions with regard to geological time." *

Even more interesting, perhaps, to many, are the results which have been obtained respecting the varying temperatures of deep-sea regions. The peculiarity just considered is, indeed, a consequence of such variations; but the fact itself is at least as interesting as the consequences which flow from it. It throws light on the long-standing controversy respecting the oceanic circulation. It has been found that the depths of the equatorial and tropical seas are colder than those of the North Atlantic. In the tropics the deep-sea temperature is considerably below the freezing-point of fresh water; in the deepest part of the Bay of Biscay the temperature is several degrees above the freezing-point. Thus one learns that the greater part of the water which lies deep below the surface of the equatorial and tropical seas comes from the Antarctic regions, though undoubtedly there are certain relatively narrow currents which carry the waters of the Arctic seas to the tropics. The great point to notice is that the water under the equatorial seas must really have travelled from polar regions. A cold of 30° can be explained in no other way. We see at once, therefore, the explanation of those westerly equatorial currents which have been so long a subject of contest. Sir John Herschel failed to prove that they are due to the trade-winds, but Maury failed equally to prove that they are due to the great warmth and consequent buoyancy of the equatorial waters. In fact, while Maury showed very convincingly that the great system of oceanic circulation is carried on *despite* the winds, Herschel proved in an equally convincing manner that the overflow conceived by Maury should result in an easterly instead of a westerly current. Recently the theory was put forward that the continual process of evaporation going on in the equatorial regions leads to an indraught of cold water in bottom-currents from the polar seas. Such currents coming *toward* the equator, that is, travelling from latitudes where the earth's eastwardly motion is less to latitudes in which that motion is greater, would lag behind, that is, would have a westwardly motion. It seems now placed beyond a doubt that this is the true explanation of the equatorial ocean-currents.

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* This opinion Dr. Carpenter has since somewhat modified. It will be remembered, of course, that the evidence derived from the nature of superposed strata is in no way affected by what is shown above to hold as respects adjacent deposits.

Such are a few, and but a few, among the many interesting results which have followed from the recent researches of Dr. Carpenter and Professor Thomson into the hitherto little known depths of the great sea.

THE TUNNEL THROUGH MONT CENIS.

Men flash their messages across mighty continents and beneath the bosom of the wide Atlantic; they weigh the distant planets, and analyze the sun and the stars; they span Niagara with a railway bridge, and pierce the Alps with a railway tunnel: yet the poet of the age in which all these things are done or doing sings, "We men are a puny race." And certainly, the great works which belong to man as a race can no more be held to evidence the importance of the individual man than the vast coral reefs and atolls of the Pacific can be held to evidence the working-power of the individual coral polype. But if man, standing alone, is weak, man working according to the law assigned to his race from the beginning — that is, in fellowship with his kind — is verily a being of power.

Perhaps no work ever undertaken by men strikes one as more daring than the attempt to pierce the Alps with a tunnel. Nature seems to have upreared these mighty barriers as if with the design of showing man how weak he is in her presence. Even the armies of Hannibal and Napoleon seemed all but powerless in the face of these vast natural fastnesses. Compelled to creep slowly and cautiously along the difficult and narrow ways which alone were open to them, decimated by the chilling blasts which swept the face of the rugged mountain-range, and dreading at every moment the pitiless swoop of the avalanche, the French and Carthaginian troops exhibited little of the pomp and dignity which we are apt to associate with the operations of warlike armies. Had the denizen of some other planet been able to watch their progress, he might indeed have said, "These men are a puny race." In this only, that they succeeded, did the troops of Hannibal and Napoleon assert the dignity of the human race. Grand as was the aspect of Nature, and mean as was that of man during the progress of the contest, it was Nature that was conquered — man that overcame. And now man has entered on a new conflict with Nature in the gloomy fastnesses of the Alps. The barrier which he had scaled of old he has now undertaken to pierce. And the work — bold and daring as it seems — is three parts finished.

The Mont Cenis tunnel was sanctioned by the Sardinian Government in 1857, and arrangements were made for fixing the perforating machinery in the years 1858 and 1859. But the work was not actually commenced until November, 1860. The tunnel, which will be fully seven and a half miles in length, was to be completed in twenty-five years. The entrance to the tunnel on the side of France is near the little village of Fourneau, and lies 3,946 feet above the level of the sea. The entrance on the side of Italy is in a deep valley at Bardonèche, and lies 4,380 feet above the sea-level. Thus there is a difference of level of 434 feet. But the tunnel will actually rise 445 feet above the level of the French end, attaining this height at a distance of about four miles from that extremity; in the remaining three and three-quarter miles there will be a fall of only ten feet, so that this part of the line will be practically level.

The rocks through which the excavations have been made have been for the most part very difficult to work. Those who imagine that the great mass of our mountain-ranges consists of such granite as is made use of in our buildings, and is uniform in texture and hardness, greatly underrate the difficulties with which the engineers of this gigantic work have had to contend. A large part of the rock consists of a crystallized calcareous schist, much broken and contorted; and through this rock run in every direction large masses of pure quartz. It will be conceived how difficult the work has been of piercing through so diversified a substance as this. The perforating machines are calculated to work best when the resistance is uniform; and it has often happened that the unequal resistance offered to the perforators has resulted in injury to the chisels. But before the work of perforating began, enormous difficulties had to be contended with. It will be understood that, in a tunnel of such vast length, it was absolutely necessary that the perforating processes carried on from the two ends should be directed with the most perfect accuracy. It has often happened in short tunnels that a want of perfect coincidence has existed between the two halves of the work, and the tunnellers from one end have sometimes altogether failed to meet those from the other. But in a short tunnel this want of coincidence is not very important, because the two interior ends of the tunnellings cannot in any case be far removed from each other. But in the case of the Mont Cenis tunnel any inaccuracy in the direction of the two tunnellings would have been fatal to the success of the work, since when the two ought to meet it might be found that they were laterally separated by two or three hundred yards. Hence it was necessary, before the work began, to survey the intermediate country, so as to ascertain with the most perfect accuracy the bearings of one end of the tunnel from the other. "It was necessary," says the narrative of these initial labors, "to prepare accurate plans and sections for the determination of the levels, to fix the axis of the tunnel, and to 'set it out' on the mountain-top; to erect observatories and guiding-signals, solid, substantial, and true." When we remember the nature of the passes over the Cenis, we can conceive the difficulty of setting out a line of this sort over the Alpine range. The necessity of continually climbing over rocks, ravines, and precipices in passing from station to station involved difficulties which, great as they were, were as nothing when compared with the difficulties resulting from the bitter weather experienced on those rugged mountain-heights. The tempests which sweep the Alpine passes — the ever-recurring storms of rain, sleet, and driving snow, are trying to the ordinary traveller. It will be understood, therefore, how terribly they must have interfered with the delicate processes involved in surveying. It often happened that for days together no work of any sort could be done owing to the impossibility of using levels and theodolites when exposed to the stormy weather and bitter cold of these lofty passes. At length, however, the work was completed, and that with such success that the greatest deviation from exactitude was less than a single foot for the whole length of the seven and a half miles.

Equally remarkable and extensive were the labors connected with the preparatory works. New and solid roads, bridges, canals, magazines, workshops, forges, furnaces, and machinery, had to be constructed; residences had to be built for the men, and offices for the engineers; in fact, at each extremity of the tunnel a complete establishment had to be formed. Those who have traversed Mont Cenis since the works began have been perplexed by the strange appearance and character of the machinery and establishments to be seen at Modane and Fourneau. The mass of pipes and tubes, tanks, reservoirs, and machinery, which would be marvellous anywhere, has a still stranger look in a wild and rugged Alpine pass.

TORNADOES.

THE inhabitants of the earth are subjected to agencies which — beneficial doubtless in the long-run, perhaps necessary to the very existence of terrestrial races — appear, at first sight, energetically destructive. Such are — in order of destructiveness — the hurricane, the earthquake, the volcano, and the thunder-storm. When we read of earthquakes such as those which overthrew Lisbon, Callao, and Riobamba, and learn that one hundred thousand persons fell victims in the great Sicilian earthquake in 1693, and probably three hundred thousand in the two earthquakes which assailed Antioch in the years 526 and 612, we are disposed to assign at once to this devastating phenomenon the foremost place among the agents of destruction. But this judgment must be reversed when we consider that earthquakes — though so fearfully and suddenly destructive both to life and property — yet occur but seldom compared with wind-storms, while the effects of a real hurricane are scarcely less destructive than those of the sharpest shocks of earthquake. After ordinary storms long miles of the sea-coast are strewn with the wrecks of many once gallant ships, and with the bodies of their hapless crews. In the spring of 1866 there might be seen at a single view from the heights near Plymouth twenty-two shipwrecked vessels, and this after a storm, which, though severe, was but trifling compared with the hurricanes which sweep over the torrid zones, and thence — scarcely diminished in force — as far north sometimes as our own latitudes. It was in such a hurricane that the "Royal Charter" was wrecked, and hundreds of stout ships with her. In the great hurricane of 1780, which commenced at Barbadoes and swept across the whole breadth of the North Atlantic, fifty sail were driven ashore at the Bermudas, two line-of-battle ships went down at sea, and upward of twenty thousand persons lost their lives on the land. So tremendous was the force of this hurricane (Captain Maury tells us) that "the bark was blown from the trees, and the fruits of the earth destroyed; the very bottom and depths of the sea were uprooted — forts and castles were washed away, and their great guns carried in the air like chaff; houses were razed; ships wrecked; and the bodies of men and beasts lifted up in the air and dashed to pieces in the storm"— an account, however, which (though doubtless faithfully rendered by Maury from the authorities he consulted) must perhaps be accepted *cum grano,* and especially with reference to the great guns carried in the air "like chaff." *

In the gale of August, 1782, all the trophies of Lord Rodney's victory, except the "Ardent," were destroyed, two British ships-of-the-line foundered at sea, numbers of merchantmen under Admiral Graves's convoy were wrecked, and at sea alone three thousand lives were lost.

But quite recently a storm far more destructive than these swept over the Bay of Bengal. Most of our readers doubtless remember the great gale of October, 1864, in which all the ships in harbor at Calcutta were swept from their anchorage, and driven one upon another in inextricable confusion. Fearful as was the loss of life and property in Calcutta harbor, the destruction on land was greater. A vast wave swept for miles over the surrounding country, embankments were destroyed, and whole villages, with their inhabitants, were swept away. Fifty thousand souls, it is believed, perished in this fearful hurricane.

The gale which has just ravaged the Gulf of Mexico adds another to the long list of disastrous hurricanes. As we write, the effects produced by this tornado are beginning to be made known. Already its destructiveness has become but too certainly evidenced.

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* We remember to have read that in this hurricane guns which had long lain under water were washed up like mere drift upon the beach. Perhaps this circumstance grew gradually into the incredible story above recorded.

The laws which appear to regulate the generation and the progress of cyclonic storms are well worthy of careful study.

The regions chiefly infested by hurricanes are the West Indies, the southern parts of the Indian Ocean, the Bay of Bengal, and the China Seas. Each region has its special hurricane season.

In the West Indies, cyclones occur principally in August and September, when the southeast monsoons are at their height. At the same season the African southwesterly monsoons are blowing. Accordingly, there are two sets of winds, both blowing heavily and steadily from the Atlantic, disturbing the atmospheric equilibrium, and thus in all probability generating the great West-Indian hurricanes. The storms thus arising show their force first at a distance of about six or seven hundred miles from the equator, and far to the east of the region in which they attain their greatest fury. They sweep with a northwesterly course to the Gulf of Mexico, pass thence northward, and so to the northeast, sweeping in a wide curve (resembling the letter U placed thus ⊂) around the West-Indian seas, and thence travelling across the Atlantic, generally expending their fury before they reach the shores of Western Europe. This course is the storm-track (or storm - ⊂ as we shall call it). Of the behavior of the winds as they traverse this track, we shall have to speak when we come to consider the peculiarity from which these storms derive their names of "cyclones" and "tornadoes."

The hurricanes of the Indian Ocean occur at the "changing of the monsoons." "During the interregnum," writes Maury, "the fiends of the storm hold their terrific sway." Becalmed often for a day or two, seamen hear moaning sounds in the air, forewarning them of the coming storm. Then, suddenly, the winds break loose from the forces which have for a while controlled them, and "seem to rage with a fury that would break up the fountains of the deep."

In the North Indian seas hurricanes rage at the same season as in the West Indies.

In the China seas occur those fearful gales known among sailors as "typhoons," or "white squalls." These take place at the changing of the monsoons. Generated, like the West-Indian hurricanes, at a distance of some ten or twelve degrees from the equator, typhoons sweep — in a curve similar to that followed by the Atlantic storms — around the East-Indian Archipelago, and the shores of China to the Japanese Islands.

There occur land-storms, also, of a cyclonic character in the valley of the Mississippi. "I have often observed the paths of such storms," says Maury, " through the forests of the Mississippi. There the track of these tornadoes is called a 'wind-road,' because they make an avenue through the wood straight along, and as clear of trees as if the old denizens of the forest had been cleared with an axe. I have seen trees three or four feet in diameter torn up by the roots, and the top, with its limbs, lying next the hole whence the root came." Another writer, who was an eye-witness to the progress of one of these American land-storms, thus speaks of its destructive effects: "I saw, to my great astonishment, that the noblest trees of the forest were falling into pieces. A mass of branches, twigs, foliage, and dust, moved through the air, whirled onward like a cloud of feathers, and passing, disclosed a wide space filled with broken trees, naked stumps, and heaps of shapeless ruins, which marked the path of the tempest."

If it appeared, on a careful comparison of observations made in different places, that these winds swept directly along those tracks which they appear to follow, a comparatively simple problem would be presented to the meteorologist. But this is not found to be the case. At one part of a hurricane's course the storm appears to be travelling with fearful fury along the true storm-[illegible]; at another less furiously directly across the storm-track; at another, but with yet diminished force, though still fiercely, in a direction exactly opposite to that of the storm-track.

All these motions appear to be fairly accounted for by the theory that the true path of the storm is a spiral — or rather, that while the centre of disturbance continually travels onward in a widely-extended curve, the storm-wind sweeps continually around the centre of disturbance, as a whirlpool around its vortex.

And here a remarkable circumstance attracts our notice, the consideration of which points to the mode in which cyclones may be conceived to be generated. It is found, by a careful study of different observations made upon the same storm, that cyclones in the northern hemisphere *invariably* sweep round the onward travelling vortex of disturbance in *one* direction, and southern cyclones in the contrary direction. If we place a watch, face upward, upon one of the northern cyclone regions in a Mercator's chart, then the motion of the hands is *contrary* to the direction in which the cyclone whirls; when the watch is shifted to a southern cyclone region, the motion of the hands takes place in the same direction as the cyclone motion. This peculiarity is converted into the following rule-of-thumb for sailors who encounter a cyclone, and seek to escape from the region of fiercest storm: *Facing the wind, the centre or vortex of the storm lies to the right in the northern, to the left in the southern, hemisphere.* Safety lies in flying from the centre in every case save one — that is, when the sailor lies in the direct track of the advancing vortex. In this case, to fly from the centre would be to keep in the storm-track; the proper course for the sailor when thus situated is to steer for the calmer side of the storm-track. This is always the outside of the ᗡ, as will appear from a moment's consideration of the spiral curve traced out by a cyclone. Thus, if the seaman *scud before the wind* — in all other cases a dangerous expedient in a cyclone*— he will probably escape unscathed. There is, however, this danger, that the storm-track may extend to or even slightly overlap the land, in which case scudding before the gale would bring the ship upon a lee-shore. And in this way many gallant ships have, doubtless, suffered wreck.

The danger of the sailor is obviously greater, however, when he is overtaken by the storm on the inner side of the storm - ᗡ. Here he has to encounter the double force of the cyclonic whirl and of the advancing storm-system, instead of the difference of the two motions, as on the outer side of the storm-track. His chance of escape will depend on his distance from the central path of the cyclone. If near to this, it is equally dangerous for him to attempt to scud to the safer side of the track, or to beat against the wind by the shorter course which would lead him out of the storm - ᗡ on its inner side. It has been shown by Colonel Sir W. Reid that this is the quarter in which vessels have been most frequently lost.

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* A ship by scudding before the gale may — if the captain is not familiar with the laws of cyclones — *go round and round* without escaping. The ship "Charles Heddle" did this in the East Indies, going round no less than *five times*.

But even the danger of this most dangerous quarter admits of degrees. It is greatest where the storm is sweeping round the most curved part of its track, which happens in about latitude twenty-five or thirty degrees. In this case, a ship may pass twice through the vortex of the storm. Here hurricanes have worked their most destructive effects. And thus it happens that sailors dread, most of all, the part of the Atlantic near Florida and the Bahamas, and the region of the Indian Ocean which lies south of Bourbon and Mauritius.

To show how important it is that captains should understand the theory of cyclones in both hemispheres, we shall here relate the manner in which Captain J. V. Hall escaped from a typhoon of the China seas. About noon, when three days out from Macao, Captain Hall saw "a most wild and uncommon-looking halo round the sun." On the afternoon of the next day, the barometer had commenced to fall rapidly; and though, as yet, the weather was fine, orders were at once given to prepare for a heavy gale. Toward evening, a bank of cloud was seen in the southeast, but when night closed, the weather was still calm and the water smooth, though the sky looked wild and a scud was coming on from the northeast. "I was much interested," says Captain Hall, "in watching for the commencement of the gale, which I now felt sure was coming. That bank to the southeast was the meteor (cyclone) approaching us, the northeast scud the outer northwest portion of it; and when at night a strong gale came on about north, or north-northwest, I felt certain we were on its western and southwestern verge. It rapidly increased in violence; but I was pleased to see the wind veering to the northwest, as it convinced me that I had put the ship on the right track, namely, on the starboard tack, standing, of course, to the southwest. From ten A. M. to three P. M. it blew with great violence, but the ship being well prepared rode comparatively easy. The barometer was now very low, the centre of the storm passing to the northward of us, to which we might have been very near had we in the first place put the ship on the larboard tack."

But the most remarkable point of Captain Hall's account remains to be mentioned. He had gone out of his course to avoid the storm, but when the wind fell to a moderate gale he thought it a pity to lie so far from his proper course, and made sail to the northwest. "In less than two hours the barometer again began to fall and the storm to rage in heavy gusts." He bore again to the southeast, and the weather rapidly improved. There can be little doubt that but for Captain Hall's knowledge of the law of cyclones, his ship and crew would have been placed in serious jeopardy, since in the heart of a Chinese typhoon a ship has been known to be thrown on her beams-ends when not showing a yard of canvas.

If we consider the regions in which cyclones appear, the paths they follow, and the direction in which they whirl, we shall be able to form an opinion as to their origin. In the open Pacific Ocean (as its name, indeed, implies) storms are uncommon; they are infrequent also in the South Atlantic and South Indian Oceans. Around Cape Horn and the Cape of Good Hope, heavy storms prevail, but they are not cyclonic, nor are they equal in fury and frequency, Maury tells us, to the true tornado. Along the equator, and for several degrees on either side of it, cyclones are also unknown. If we turn to a map in which ocean-currents are laid down, we shall see that in every "cyclone region" there is a strongly-marked current, and that each current follows closely the track which we have denominated the storm - ⊂ . In the North Atlantic we have the great Gulf Stream, which sweeps from equatorial regions into the Gulf of Mexico, and thence across the Atlantic to the shores of Western Europe. In the South Indian Ocean there is the "south equatorial current" which sweeps past Mauritius and Bourbon, and thence returns toward the east. In the Chinese Sea, there is the north equatorial current, which sweeps round the East-Indian Archipelago, and then merges into the Japanese current. There is also the current in the Bay of Bengal, flowing through the region in which, as we have seen, cyclones are commonly met with. There are other sea-currents besides these which yet breed no cyclones. But we may notice two peculiarities in the currents we have named. They all flow from equatorial to temperate regions, and, secondly, they are all "horseshoe currents." So far as we are aware, there is but one other current which presents both these peculiarities, namely — the great Australian current between New Zealand and the eastern shores of Australia. We have not yet met with any record of cyclones occurring over the Australian current, but heavy storms are known to prevail in that region, and we believe that when these storms have been studied as closely as the storms in better-known regions, they will be found to present the true cyclonic character.

Now, if we inquire why an ocean-current travelling from the equator should be a "storm-breeder," we shall find a ready answer. Such a current, carrying the warmth of intertropical regions to the temperate zones, produces in the first place, by the mere difference of temperature, important atmospheric disturbances. The difference is so great, that Franklin suggested the use of the thermometer in the North Atlantic Ocean as a ready means of determining the longitude, since the position of the Gulf Stream at any given season is almost constant.

But the warmth of the stream itself is not the only cause of atmospheric disturbance. Over the warm water vapor is continually rising; and, as it rises, is continually condensed (like the steam from a locomotive) by the colder air round. "An observer on the moon," says Captain Maury, "would, on a winter's day, be able to trace out by the mist in the air, the path of the Gulf Stream through the sea." But what must happen when vapor is condensed? We know that to turn water into vapor is a process requiring — that is, *using up* — a large amount of heat; and, conversely the return of vapor to the state of water *sets free* an equivalent quantity of heat. The amount of heat thus set free from the Gulf Stream is thousands of times greater than that which would be generated by the whole coal-supply annually raised in Great Britain. Here, then, we have an efficient cause for the wildest hurricanes. For along the whole of the Gulf Stream, from Bemini to the Grand Banks, there is a channel of heated — that is, *rarefied air.* Into this channel the denser atmosphere on both sides is continually pouring, with greater or less strength. When a storm begins in the Atlantic, it always makes for this channel, "and, reaching it, turns and follows it in its course, sometimes entirely across the Atlantic." "The southern points of America and Africa have won for themselves," says Maury, "the name of 'the stormy capes,' but there is not a storm-fiend in the wide ocean can out-top that which rages along the Atlantic coasts of North America. The China seas and the North Pacific may vie in the fury of their gales with this part of the Atlantic, but Cape Horn and the Cape of Good Hope cannot equal them, certainly in frequency, nor do I believe, in fury." We read of a West-Indian storm so violent, that "it forced the Gulf Stream back to its sources, and piled up the water to a height of thirty feet in the Gulf of Mexico. The ship 'Ledbury Snow' attempted to ride out the storm. When it abated she found herself high up on the dry land, and discovered that she had let go her anchor among the tree-tops on Elliott's Key."

By a like reasoning we can account for the cyclonic storms prevailing in the North Pacific Ocean. Nor do the tornadoes which rage in parts of the United States present any serious difficulty. The region along which these storms travel is the valley of the great Mississippi. This river at certain seasons is considerably warmer than the surrounding lands. From its surface, also, aqueous vapor is continually being raised. When the surrounding air is colder, this vapor is presently condensed, generating in the change a vast amount of heat. We have thus a channel of rarefied air over the Mississippi Valley, and this channel becomes a storm-track, like the corresponding channels over the warm ocean-currents. The extreme violence of land-storms is probably due to the narrowness of the track within which they are compelled to travel. For it has been noticed that the fury of a sea-cyclone increases as the range of the "whirl" diminishes, and *vice versa.*

There seems, however, no special reason why cyclones should follow the storm - ⊲ in one direction rather than in the other. We must, to understand this, recall the fact that under the torrid zones the conditions necessary for the generation of storms prevail far more intensely than in temperate regions. Thus the probability is far greater that cyclones should be generated at the tropical than at the temperate end of the storm - ⊲. Still it is worthy of notice, that in the land-locked North Pacific Ocean, true typhoons *have* been known to follow the storm-track in a direction contrary to that commonly noticed.

The direction in which a true tornado *whirls* is *invariably* that we have mentioned. The explanation of this peculiarity would occupy more space than we can here afford. Those readers who may wish to understand the origin of the law of cyclonic rotation should study Herschel's interesting work on Meteorology.

The suddenness with which a true tornado works destruction was strikingly exemplified in the wreck of the steamship "San Francisco." She was assailed by an extra tropical tornado when about 300 miles from Sandy Hook, on December 24, 1853. In a few moments she was a complete wreck! The wide range of a tornado's destructiveness is shown by this, that Colonel Reid examined one along whose track no less than 110 ships were wrecked, crippled, or dismasted.

VESUVIUS.

The eruption in progress, as we write, from Mount Vesuvius, and the numerous and violent eruptions from this mountain during the last two centuries, seem to afford an answer to those who think there are traces of a gradually diminishing activity in the earth's internal forces. That such a diminution is taking place, we may admit; but that its rate of progress is perceptible — that we can point to a time within the historical epoch, nay even within the limits of geological evidence, at which the earth's internal forces were *certainly* more active than they are at the present time — may, we think, be denied absolutely.

When the science of geology was but young, and its professors sought to compress within a few years (at the outside) a series of events which (we now know) must have occupied many centuries, there was room, indeed, for the supposition that modern volcanic eruptions, as compared with ancient outbursts, are but as the efforts of children compared with the work of giants. And accordingly, we find a distinguished French geologist writing, even so late as 1829, that in ancient times "tous les phénomènes géologiques se passaient dans des dimensions *centuples* de celles qu'ils présentent aujourd'hui." But now we have such certain evidence of the enormous length of the intervals within which volcanic regions assumed their present appearance — we have such satisfactory means of determining which of the events occurring within those intervals were or were not contemporary — that we are safe from the error of assuming that Nature at a single effort fashioned widely-extended districts just as we now see them. And accordingly, we have the evidence of one of the most distinguished of living geologists, that there is no volcanic mass "of ancient date, distinctly referable to a single eruption, which can even *rival* in volume the matter poured out from Skaptâr Jokul in 1783."

In the volcanic region of which Vesuvius or Somma is the principal vent, we have a remarkable instance of the deceptive nature of that state of rest into which some of the principal volcanoes frequently fall for many centuries together. For how many centuries before the Christian era Vesuvius had been at rest, is not known; but this is certain, that, from the landing of the first Greek colony in Southern Italy, Vesuvius gave no signs of internal activity. It was recognized by Strabo as a volcanic mountain, but Pliny did not include it in the list of active volcanoes. In those days, the mountain presented a very different appearance from that which it now exhibits. In place of the two peaks now seen, there was a single, somewhat flattish, summit, on which a slight depression marked the place of an ancient crater. The fertile slopes of the mountain were covered with well-cultivated fields, and the thriving cities Herculaneum, Pompeii, and Stabiæ, stood near the base of the sleeping mountain. So little did any thought of danger suggest itself in those times, that the bands of slaves, murderers, and pirates which flocked to the standard of Spartacus found a refuge, to the number of many thousands, within the very crater itself.

But though Vesuvius was at rest, the region of which Vesuvius is the main vent was far from being so. The island of Pithecusa (the modern Ischia) was shaken by frequent and terrible convulsions. It is even related that Prochyta (the modern Procida) was rent from Pithecusa in the course of a tremendous upheaval, though Pliny derives the name Prochyta (or "poured forth") from the supposed fact of this island having been poured forth by an eruption from Ischia. Far more probably, Prochyta was formed independently by submarine eruptions, as the volcanic islands near Santorin have been produced in more recent times.

So fierce were the eruptions from Pithecusa, that several Greek colonies which attempted to settle on this island were compelled to leave it. About 380 years before the Christian era, colonists under King Hiero of Syracuse, who had built a fortress on Pithecusa, were driven away by an eruption. Nor were eruptions the sole cause of danger. Poisonous vapors, such as are emitted by volcanic craters after eruption, appear to have exhaled, at times, from extensive tracts on Pithecusa, and thus to have rendered the island uninhabitable.

Still nearer to Vesuvius lay the celebrated Lake Avernus. The name Avernus is said to be a corruption of the Greek word *Aornos,* signifying "without birds," the poisonous exhalations from the waters of the lake destroying all birds which attempted to fly over its surface. Doubt has been thrown on the destructive properties assigned by the ancients to the vapors ascending from Avernus. The lake is now a healthy and agreeable neighborhood, frequented, says Humboldt, by many kinds of birds, which suffer no injury whatever, even when they skim the very surface of the water. Yet there can be little doubt that Avernus hides the outlet of an extinct volcano; and long after this volcano had become inactive, the lake which concealed its site "may have deserved the appellation of 'atri janua Ditis,' emitting, perhaps, gases as destructive of animal life as those suffocating vapors given out by Lake Quilotoa, in Quito, in 1797, by which whole herds of cattle were killed on its shores, or as those deleterious emanations which annihilated all the cattle in the island of Lancerote, one of the Canaries, in 1730."

While Ischia was in full activity, not only was Vesuvius quiescent, but even Etna seemed to be gradually expiring, so that Seneca ranks this volcano among the number of nearly-extinguished craters. At a later epoch, Ælian asserted that the mountain itself was sinking, so that seamen lost sight of the summit at a less distance across the seas than of old. Yet within the last two hundred years there have been eruptions from Etna rivalling, if not surpassing, in intensity the convulsions recorded by ancient historians.

We shall not here attempt to show that Vesuvius and Etna belong to the same volcanic system, though there is reason not only for supposing this to be the case, but for the belief that all the subterranean regions whose effects have been shown from time to time over the district extending from the Canaries and the Azores, across the whole of the Mediterranean, and into Syria itself, belong to but one great centre of internal action. But it is quite certain that Ischia and Vesuvius are outlets from a single source.

While Vesuvius was dormant, resigning for a while its pretensions to be the principal vent of the great Neapolitan volcanic system, Ischia, we have seen, was rent by frequent convulsions. But the time was approaching when Vesuvius was to resume its natural functions, and with all the more energy that they had been for a while suspended.

In the year 63 (after Christ) there occurred a violent convulsion of the earth around Vesuvius, during which much injury was done to neighboring cities, and many lives were lost. From this period shocks of earthquake were felt from time to time for sixteen years. These grew gradually more and more violent, until it began to be evident that the volcanic fires were about to return to their main vent. The obstruction which had so long impeded the exit of the confined matter was not, however, readily removed, and it was only in August of the year 79, after numerous and violent internal throes, that the superincumbent mass was at length hurled forth. Rocks and cinders, lava, sand, and scoriæ, were propelled from the crater, and spread many miles on every side of Vesuvius.

We have an interesting account of the great eruption which followed in a letter from the younger Pliny to the younger Tacitus. The latter had asked for an account of the death of the elder Pliny, who lost his life in his eagerness to obtain a near view of the dreadful phenomenon. "He was at that time," says his nephew, "with the fleet under his command at Misenum. On August 24th, about one in the afternoon, my mother desired him to observe a cloud of very extraordinary size and shape. He had just returned from taking the benefit of the sun, and, after bathing himself in cold water, and taking a slight repast, had retired to his study. He arose at once, and went out upon a height whence he might more distinctly view this strange phenomenon. It was not at this distance discernible from what mountain the cloud issued, but it was found afterward that it came from Vesuvius. I cannot give a more exact description of its figure than by comparing it to that of a pine-tree, for it shot up to a great height in the form of a trunk, which extended itself at the top into a sort of branches; occasioned, I suppose, either by a sudden gust of air which impelled it, whose force decreased as it advanced upward, or else the cloud itself, being pressed back by its own weight, expanded in this manner. The cloud appeared sometimes bright, at others dark and spotted, as it was more or less impregnated with earth and cinders."

These extraordinary appearances attracted the curiosity of the elder Pliny. He ordered a small vessel to be prepared, and started to seek a nearer view of the burning mountain. His nephew declined to accompany him, being engaged with his studies. As Pliny left the house, he received a note from a lady whose house, being at the foot of Vesuvius, was in imminent danger of destruction. He set out, accordingly, with the design of rendering her assistance, and also of assisting others, "for the villas stood extremely thick upon that lovely coast." He ordered the galleys to be put to sea, and steered directly to the point of danger, so cool in the midst of the turmoil around "as to be able to make and dictate observations upon the motions and figures of that dreadful scene." As he approached Vesuvius, cinders, pumice-stones, and black fragments of burning rock, fell on and around the ships. "They were in danger, too, of running aground, owing to the sudden retreat of the sea; vast fragments, also, rolled down from the mountain and obstructed all the shore." The pilot advising retreat, Pliny made the noble answer, "Fortune befriends the brave," and bade him press onward to Stabiæ. Here he found his friend Pomponianus in great consternation, already prepared for embarking, and waiting only for a change in the wind. Exhorting Pomponianus to be of good courage, Pliny quietly ordered baths to be prepared; and "having bathed, sat down to supper with great cheerfulness, or at least (which is equally heroic) with all the appearance of it." Assuring his friends that the flames which appeared in several places were merely burning villages, Pliny presently retired to rest, and "being pretty fat," says his nephew, "and breathing hard, those who attended without actually heard him snore." But it became necessary to awaken him, for the court which led to his room was now almost filled with stones and ashes. He got up and joined the rest of the company, who were consulting on the propriety of leaving the house, now shaken from side to side by frequent concussions. They decided on seeking the fields for safety; and fastening pillows on their heads, to protect them from falling stones, they advanced in the midst of an obscurity greater than that of the darkest night — though beyond the limits of the great cloud it was already broad day. When they reached the shore, they found the waves running too high to suffer them safely to venture to put out to sea. Pliny, "having drunk a draught or two of cold water, lay down on a cloth that was spread out for him; but at this moment the flames and sulphurous vapors dispersed the rest of the company and obliged him to rise. Assisted by two of his servants, he got upon his feet, but instantly fell down dead; suffocated, I suppose," says his nephew, "by some gross and noxious vapor, for he always had weak lungs and suffered from a difficulty of breathing." His body was not found until the third day after his death, when for the first time it was light enough to search for him. He was found as he had fallen, "and looking more like a man asleep than dead."

But even at Misenum there was danger, though Vesuvius is distant no less than fourteen miles. The earth was shaken with repeated and violent shocks, "insomuch," says the younger Pliny, "that they threatened our complete destruction." When morning came, the light was faint and glimmering; the buildings around seemed tottering to their fall, and, standing on the open ground, the chariots which Pliny had ordered were so agitated backward and forward that it was impossible to keep them steady, even by supporting them with large stones. The sea was rolled back upon itself, and many marine animals were left dry upon the shore. On the side of Vesuvius, a black and ominous cloud, bursting with sulphurous vapors, darting out long trains of fire, resembling flashes of lightning, but much larger. Presently the great cloud spread over Misenum and the island of Capreæ. Ashes fell around the fugitives. On every side "nothing was to be heard but the shrieks of women and children, and the cries of men: some were calling for their children, others for their parents, others for their husbands, and only distinguishing each other by their voices: one was lamenting his own fate, another that of his family; some wished to die, that they might escape the dreadful fear of death; but the greater part imagined that the last and eternal night was come, which was to destroy the gods and the world together." At length a light appeared, which was not, however, the day, but the forerunner of an outburst of flames. These presently disappeared, and again the thick darkness spread over the scene. Ashes fell heavily upon the fugitives, so that they were in danger of being crushed and buried in the thick layer rapidly covering the whole country. Many hours passed before the dreadful darkness began slowly to be dissipated. When at length day returned, and the sun was seen faintly shining through the overhanging canopy of ashes, "every object seemed changed, being covered over with white ashes as with a deep snow."

It is remarkable that Pliny makes no mention in his letter of the destruction of the two populous and important cities, Pompeii and Herculaneum. We have seen that at Stabiæ a shower of ashes fell so heavily that several days before the end of the eruption the court leading to the elder Pliny's room was beginning to be filled up; and when the eruption ceased, Stabiæ was completely overwhelmed. Far more sudden, however, was the destruction of Pompeii and Herculaneum.

It would seem that the two cities were first shaken violently by the throes of the disturbed mountain. The signs of such a catastrophe have been very commonly assigned to the earthquake which happened in 63, but it seems far more likely that most of them belong to the days immediately preceding the great outburst in 79. "In Pompeii," says Sir Charles Lyell, "both public and private buildings bear testimony to the catastrophe. The walls are rent, and in many places traversed by fissures still open." It is probable that the inhabitants were driven by these anticipatory throes to fly from the doomed towns. For though Dion Cassius relates that "two entire cities, Herculaneum and Pompeii, were buried under showers of ashes, while all the people were sitting in the theatre," yet "the examination of the two cities enables us to prove," says Sir Charles, "that none of the people were destroyed in the theatre, and, indeed, that there were very few of the inhabitants who did not escape from both cities. Yet," he adds, "some lives were lost, and there was ample foundation for the tale in all its most essential particulars."

We may note here, in passing, that the account of the eruption given by Dion Cassius, who wrote a century and a half after the catastrophe, is sufficient to prove how terrible an impression had been made upon the inhabitants of Campania, from whose descendants he in all probability obtained the materials of his narrative. He writes that, "during the eruption, a multitude of men of superhuman stature, resembling giants, appeared, sometimes on the mountain, and sometimes in the environs; that stones and smoke were thrown out, the sun was hidden, and then the giants seemed to rise again, while the sounds of trumpets were heard"— with much other matter of a similar sort.

In the great eruption of 79, Vesuvius poured forth lapilli, sand, cinders, and fragments of old lava, but no new lava flowed from the crater. Nor does it appear that any lava-stream was ejected during the six eruptions which took place during the following ten centuries. In the year 1036, for the first time, Vesuvius was observed to pour forth a stream of molten lava. Thirteen years later, another eruption took place; then 90 years passed without disturbance, and after that a long pause of 168 years. During this interval, however, the volcanic system of which Vesuvius is the main but not the only vent, had been disturbed twice. For it is related than in 1198 the Solfatara Lake crater was in eruption; and in 1302, Ischia, dormant for at least 1,400 years, showed signs of new activity. For more than a year earthquakes had convulsed this island from time to time, and at length the disturbed region was relieved by the outburst of a lava stream from a new vent on the southeast of Ischia. The lava-stream flowed right down to the sea, a distance of two miles. For two months, this dreadful outburst continued to rage; many houses were destroyed; and although the inhabitants of Ischia were not completely expelled, as happened of old with the Greek colonists, yet a partial emigration took place.

The next eruption of Vesuvius occurred in 1306; and then three centuries and a quarter passed during which only one eruption, and that an unimportant one (in 1500), took place. "It was remarked," says Sir Charles Lyell, "that throughout this long interval of rest Etna was in a state of unusual activity, so as to lend countenance to the idea that the great Sicilian volcano may sometimes serve as a channel of discharge to elastic fluids and lava that would otherwise rise to the vents in Campania."

Nor was the abnormal activity of Etna the only sign that the quiescence of Vesuvius was not to be looked upon as any evidence of declining energy in the volcanic system. In 1538 a new mountain was suddenly thrown up in the Phlegræan Fields — a district including within its bounds Pozzuoli, Lake Avernus, and the Solfatara. The new mountain was thrown up near the shores of the Bay of Baiæ. It is 440 feet above the level of the bay, and its base is about a mile and a half in circumference.

The depth of the crater is 421 feet, so that its bottom is only six yards above the level of the bay. The spot on which the mountain was thrown up was formerly occupied by the Lucrine Lake; but the outbursts filled up the greater part of the lake, leaving only a small and shallow pool.

The accounts which have reached us of the formation of this new mountain are not without interest. Falconi, who wrote in 1538, mentions that several earthquakes took place during the two years preceding the outburst, and above twenty shocks on the day and night before the eruption. "The eruption began on September 29, 1538. It was on a Sunday, about one o'clock in the night, when flames of fire were seen between the hot-baths and Tripergola. In a short time the fire increased to such a degree that it burst open the earth in this place, and threw up a quantity of ashes and pumice-stones, mixed with water, which covered the whole country. The next morning the poor inhabitants of Pozzuoli quitted their habitations in terror, covered with the muddy and black shower, which continued the whole day in that country — flying from death, but with death painted in their countenances. Some with their children in their arms, some with sacks full of their goods; others leading an ass, loaded with their frightened family, toward Naples. . . . The sea had retired on the side of Baiæ, abandoning a considerable tract; and the shore appeared almost entirely dry, from the quantity of ashes and broken pumice-stones thrown up by the eruption."

Pietro Giacomo di Toledo gives us some account of the phenomena which preceded the eruption; "That plain which lies between Lake Avernus, the Monte Barbaro, and the sea, was raised a little, and many cracks were made in it, from some of which water issued; at the same time the sea immediately adjoining the plain dried up about two hundred paces, so that the fish were left on the sand, a prey to the inhabitants of Pozzuoli. At last, on September 29th, about two o'clock in the night, the earth opened near the lake, and discovered a horrid mouth, from which were furiously vomited smoke, fire, stones, and mud composed of ashes, making at the time of the opening a noise like the loudest thunder. The stones which followed were by the flames converted to pumice, and some of these were *larger than an ox*. The stones went about as high as a cross-bow will carry, and then fell down, sometimes on the edge, and sometimes into the mouth itself. The mud was of the color of ashes, and at first very liquid, then by degrees less so; and in such quantities that in less than twelve hours, with the help of the above-mentioned stones, a mountain was raised of 1,000 paces in height. Not only Pozzuoli and the neighboring country were full of this mud, but the city of Naples also; so that many of its palaces were defaced by it. This eruption lasted two nights and two days without intermission, though not always with the same force; the third day the eruption ceased, and I went up with many people to the top of the new hill, and saw down into its mouth, which was a round cavity about a quarter of a mile in circumference, in the middle of which the stones which had fallen were boiling up just as a caldron of water boils on the fire. The fourth day it began to throw up again, and the seventh day much more, but still with less violence than the first night. At this time many persons who were on the hill were knocked down by the stones and killed, or smothered with the smoke."

And now, for nearly a century, the whole district continued in repose. Nearly five centuries had passed since there had been any violent eruption of Vesuvius itself; and the crater seemed gradually assuming the condition of an extinct volcano. The interior of the crater is described by Bracini, who visited Vesuvius shortly before the eruption of 1631, in terms that would have fairly represented its condition before the eruption of 79: "The crater was five miles in circumference, and about a thousand paces deep; its sides were covered with brushwood, and at the bottom there was a plain on which cattle grazed. In the woody parts, wild boars frequently harbored. In one part of the plain, covered with ashes, were three small pools, one filled with hot and bitter water, and another salter than the sea, and a third hot, but tasteless." But in December, 1631, the mountain blew away the covering of rock and cinders which supported these woods and pastures. Seven streams of lava poured from the crater, causing a fearful destruction of life and property. Resina, built over the site of Herculaneum, was entirely consumed by a raging lava-stream. Heavy showers of rain, generated by the steam evolved during the eruption, caused in their turn an amount of destruction scarcely less important than that resulting from the lava-streams. For, falling upon the cone, and sweeping thence large masses of ashes and volcanic dust, these showers produced destructive streams of mud, consistent enough to merit the name of "aqueous lava" commonly assigned to it.

An interval of thirty-five years passed before the next eruption. But, since 1666, there has been a continual series of eruptions, so that the mountain has scarcely ever been at rest for more than ten years together. Occasionally there have been two eruptions within a few months; and it is well worthy of remark that, during the three centuries which have elapsed since the formation of Monte Nuovo, there has been no volcanic disturbance in any part of the Neapolitan volcanic district save in Vesuvius alone. Of old, as Brieslak well remarks, there had been irregular disturbances in some part of the Bay of Naples once in every two hundred years; the eruption of Solfatara in the twelfth century, that of Ischia in the fourteenth, and that of Monte Nuovo in the sixteenth; but "the eighteenth has formed an exception to the rule." It seems clear that the constant series of eruptions from Vesuvius during the past two hundred years has sufficed to relieve the volcanic district of which Vesuvius is the principal vent.

Of the eruptions which have disturbed Vesuvius during the last two centuries, those of 1779, 1793, and 1822, are in some respects the most remarkable.

Sir William Hamilton has given a very interesting account of the eruption of 1779. Passing over those points in which this eruption resembled others, we may note its more remarkable features. Sir William Hamilton says, that in this eruption molten lava was thrown up in magnificent jets to the height of at least 10,000 feet. Masses of stones and scoriæ were to be seen propelled along by these lava-jĕts. Vesuvius seemed to be surmounted by an enormous column of fire. Some of the jets were directed by the wind toward Ottajano; others fell on the cone of Vesuvius, on the outer circular mountain Somma, and on the valley between. Falling, still red hot and liquid, they covered a district more than two miles and a half wide with a mass of fire. The whole space above this district, to the height of 10,000 feet, was filled also with the falling and rising lava-streams; so that there was continually present a body of fire covering the extensive space we have mentioned, and extending nearly two miles high. The heat of this enormous fire-column was distinctly perceptible at a distance of at least six miles on every side.

The eruption of 1793 presented a different aspect. Dr. Clarke tells us that millions of red-hot stones were propelled into the air to at least half the height of the cone itself; then turning, they fell all around in noble curves. They covered nearly half the cone of Vesuvius with fire. Huge masses of white smoke were vomited forth by the disturbed mountain, and formed themselves, at a height of many thousands of feet above the crater, into a huge, ever-moving canopy, through which, from time to time, were hurled pitch-black jets of volcanic dust, and dense vapors, mixed with cascades of red-hot rocks and scoriæ. The rain which fell from the cloud-canopy was scalding hot.

Dr. Clarke was able to compare the different appearances presented by the lava when it burst from the very mouth of the crater, and lower down when it had approached the plain. As it rushed forth from its imprisonment, it streamed a liquid, white, and brilliantly pure river, which burned for itself a smooth channel through a great arched chasm in the side of the mountain. It flowed with the clearness of "honey in regular channels, cut finer than art can imitate, and glowing with all the splendor of the sun. Sir William Hamilton had conceived," adds Dr. Clarke, "that stones thrown upon a current lava would produce no impression. I was soon convinced of the contrary. Light bodies, indeed, of five, ten, and fifteen pounds' weight, made little or no impression, even at the source; but bodies of sixty, seventy, and eighty pounds were seen to form a kind of bed on the surface of the lava, and floated away with it. A stone of three hundredweight, that had been thrown out by the crater, lay near the source of the current of lava. I raised it up on one end, and then let it fall in upon the liquid lava, when it gradually sank beneath the surface and disappeared. If I wished to describe the manner in which it acted upon the lava, I should say that it was like a loaf of bread thrown into a bowl of very thick honey, which gradually involves itself in the heavy liquid, and then slowly sinks to the bottom."

But, as the lava flowed down the mountain-slopes, it lost its brilliant whiteness; a crust began to form upon the surface of the still molten lava, and this crust broke into innumerable fragments of porous matter, called scoriæ. Underneath this crust — across which Dr. Clarke and his companions were able to pass without other injury than the singeing of their boots — the liquid lava still continued to force its way onward and downward past all obstacles. On its arrival at the bottom of the mountain, says Dr. Clarke, "the whole current," encumbered with huge masses of scoriæ, "resembled nothing so much as a heap of unconnected cinders from an iron-foundry," "rolling slowly along," he says in another place, "and falling with a rattling noise over one another."

After the eruption described by Dr. Clarke, the great crater gradually filled up. Lava boiled up from below, and small craters, which formed themselves over the bottom and sides of the great one poured forth lava loaded with scoriæ. Thus, up to October, 1822, there was to be seen, in place of a regular crateriform opening, a rough and uneven surface, scored by huge fissures, whence vapor was continually being poured, so as to form clouds above the hideous heap of ruins. But the great eruption of 1822 not only flung forth all the mass which had accumulated within the crater, but wholly changed the appearance of the cone. An immense abysm was formed, three-quarters of a mile across, and extending 2,000 feet downward into the very heart of Vesuvius. Had the lips of the crater remained unchanged, indeed, the depth of this great gulf would have been far greater. But so terrific was the force of the explosion that the whole of the upper part of the cone was carried clean away, and the mountain reduced in height by nearly a full fifth of its original dimensions. From the time of its formation the chasm gradually filled up; so that, when Mr. Scrope saw it soon after the eruption, its depth was reduced by more than 1,000 feet.

Of late, Vesuvius has been as busy as ever. In 1833 and 1834 there were eruptions; and it is but twelve years since a great outburst took place. Then, for three weeks together, lava streamed down the mountain-slopes. A river of molten lava swept away the village of Cercolo, and ran nearly to the sea at Ponte Maddaloni. There were then formed ten small craters within the great one. But these have now united, and pressure from beneath has formed a vast cone where they had been. The cone has risen above the rim of the crater, and as we write torrents of lava are being poured forth. At first the lava formed a lake of fire, but the seething mass found an outlet, and poured in a wide stream toward Ottajano. Masses of red-hot stone and rock are hurled forth, and a vast canopy of white vapor hangs over Vesuvius, forming at night, when illuminated by the raging mass below, a glory of resplendent flame around the summit of the mountain.

It may seem strange that the neighborhood of so dangerous a mountain should be inhabited by races free to choose more peaceful districts. Yet, though Herculaneum, Pompeii, and Stabiæ, lie buried beneath the lava and ashes thrown forth by Vesuvius, Portici and Resina, Torre del Greco and Torre dell' Annunziata have taken their place; and a large population, cheerful and prosperous, flourish around the disturbed mountain, and over the district of which it is the somewhat untrustworthy safety-valve.

It has, indeed, been well pointed out by Sir Charles Lyell that "the general tendency of subterranean movements, when their effects are considered for a sufficient lapse of ages, is eminently beneficial, and that they constitute an essential part of that mechanism by which the integrity of the habitable surface is preserved. Why the working of this same machinery should be attended with so much evil, is a mystery far beyond the reach of our philosophy, and must probably remain so until we are permitted to investigate, not our planet alone and its inhabitants, but other parts of the moral and material universe with which they may be connected. Could our survey embrace other worlds, and the events, not of a few centuries only, but of periods as indefinite as those with which geology renders us familiar, some apparent contradictions might be reconciled, and some difficulties would doubtless be cleared up. But even then, as our capacities are finite, while the scheme of the universe may be infinite, both in time and space, it is presumptuous to suppose that all sources of doubt and perplexity would ever be removed. On the contrary, they might, perhaps, go on augmenting in number, although our confidence in the wisdom of the plan of Nature should increase at the same time; for it has been justly said" (by Sir Humphry Davy) "that the greater the circle of light, the greater the boundary of darkness by which it is surrounded."

THE EARTHQUAKE IN PERU.

The intelligence published last Saturday is sufficient to prove that the great earthquake which has devastated Peru fully equalled, if it did not surpass, the most terrible catastrophes which have ever befallen that country. It presents, too, all the features which have hitherto characterized earthquakes in that neighborhood. These are well worthy of careful study, and appear to have an important bearing on the modern theory of earthquakes.

It has been commonly held that the seat of disturbance in the earthquakes which have shaken the country west of the Andes has lain always at some point or other beneath that range of mountains. The fact that several large volcanoes are found in the Cordilleras has seemed confirmatory of this view. The accounts we have also of the great earthquake at Riobamba in 1797, seem only explicable by supposing that the seat of disturbance lay almost immediately beneath that city. The inhabitants were flung vertically upward into the air, and to such a height that Humboldt found the skeletons of many of them on the summit of the hill La Culca, on the farther side of the small river on which Riobamba is built. The ruins of many houses were also flung to the same spot. Here, therefore, was evidence of that vertical (or, as Humboldt expresses it, explosive) force which is only to be looked for immediately above the centre of concussion.

Yet the consideration of the evidence afforded by the news we have just published, seems at first sight somewhat opposed to this view, and to point rather to a seat of disturbance lying considerably to the west of the Peruvian shores. "At Chala," says our informant, "the sea receded, and a wave rose fifty feet, and returned, spreading into the town a distance of about a thousand feet. Three successive times everything within range was swept away, followed by twelve shocks of earthquake, lasting from three seconds to two minutes." The arrival of great sea-waves before the land-shocks were felt seems decisively to indicate that the seat of disturbance lay beneath the ocean and not beneath the land. We are disposed to believe, however, that in the confusion of mind naturally resulting from the occurrence of so terrible a catastrophe, the sequence of events may not have been very closely attended to, for in other places the arrival of the great sea-wave is distinctly described as following the occurrence of the earth-shock. At Arica, for example, a considerable interval would seem to have elapsed before the terrible sea-wave, which has always characterized Peruvian earthquakes, poured in upon the town. The agent of the Pacific Steam Navigation Company, whose house had been destroyed by the earth-shock, saw the great sea-wave while he was flying toward the hills. He writes: "While passing toward the hills, with the earth shaking, a great cry went up to heaven. The sea had retired. On clearing the town, I looked back and saw that the vessels were being carried irresistibly seaward. In a few minutes the sea stopped, and then arose a mighty wave fifty feet high, and came in with a fearful rush, carrying every thing before it in terrible majesty. The whole of the shipping came back, speeding toward inevitable doom. In a few minutes all was completed — every vessel was either on shore or bottom upward." This, then, was undoubtedly the great sea-wave, as compared with the minor waves of disturbance which characterize all earthquakes near the shores of the ocean.

One remarkable feature in this terrible earthquake is the enormous range of country affected by it. From Quito southward as far as Iquique — or, in other words, for a distance considerably exceeding a full third part of the whole length of the South American Andes — the shock was felt with the most terrible distinctness. We have yet to learn how much farther to the north and south, and how far inland on the eastern slopes of the Andes, the shock was experienced. But there can be little doubt that the disturbed country was equal to at least a fourth of Europe.

The portion of the Andes thus disturbed seems to be distinct from the part to which the great Chilian earthquakes belong. The difference in character between the Peruvian and Chilian earthquakes is a singular and interesting phenomenon. The difference corresponds to a feature long since pointed out by Sir Charles Lyell — the alternation, on a grand scale, of districts of active with those of extinct volcanoes. It is said that in Chili a year scarcely ever passes without shocks of earthquake being felt; in certain regions, not even a month. A similar persistence of earthquake disturbance characterizes Peru. Yet, although both districts are shaken in this manner, there seems to be a distinct evidence of alternating disturbance as respects the occurrence of great earthquakes. Thus, in 1797, took place the terrible earthquake of Riobamba. Then, thirty years later, a series of great earthquakes shook Chili, permanently elevating the whole line of coast to the height of several feet. Now, again, after another interval of about thirty years, the Andes are disturbed by a great earthquake, and this time it is the Peruvian Andes which experience the shock. Between Chili and Peru there is a space upward of five hundred miles long, in which no volcanic action has been observed. Singularly enough, this very portion of the Andes, to which one would imagine the Peruvians and Chilians would fly as to a region of safety, is the part most thinly inhabited, insomuch that, as Von Buch observes, it is in some places entirely deserted.

Near Quito the trembling of the earth is almost incessant, according to M. Boussingault. He considers that the frequency of the movement is due rather to the continual falling in of masses of rock which have been fractured in recent earthquakes, than to the persistence of subterranean action. He adds that the height of several mountains in the Andes has diminished in modern times. He refers, doubtless, to the Peruvian and Columbian Andes, and not to the Chilian. In the latter portion of the range there must be a continual increase of height, since each earthquake in Chili has produced a perceptible recession of the sea. Darwin, indeed, relates that near Valparaiso he saw beds of sea-shells belonging to recent species at a height of about a quarter of a mile above the present sea-level; and he concluded that the land had been raised to this height by a series of such small elevations as were observed to have taken place during the earthquakes of 1822, 1835, and 1837. That a contrary process should be going on in Peru, confirms the idea that a sort of undulatory or balancing motion is taking place — one long stretch of the Cordilleras rising while another is sinking. A tradition prevails among the Indians of Lican that the mountain called L'Altar, or Cassac Urcu — which means "the chief"—was once the highest of the sub-equatorial Andes, being higher even than Chimborazo; but, adds the tradition, in the reign of Quainia Abomatha, before the discovery of America, a prodigious eruption took place which lasted no less than eight years, and brought down the summit of the mountain. M. Boussingault states that the fragments of trachyte which once formed the summit of this celebrated mountain are now spread over the plain. At present Cotopaxi is the loftiest volcano of the Cordilleras, its height being no less than 18,858 feet. No mountain has ever been the seat of such terrible and destructive eruptions as those which have burst forth from Cotopaxi. The intensity of the heat which prevails during eruption will be readily gathered from the circumstance that in January, 1803, the enormous bed of snow which usually covers the cone of the volcano was dissolved in a single night.

It would seem that the Mexican volcanoes also belong to the same region of disturbances. Near the Isthmus of Panama the great Cordillera of the Andes lowers itself to the height of about 800 feet, and beyond begins the continuation of the volcanic chain in Central America and Mexico. Nor are the volcanoes of the West Indian or Caribbee Islands wholly disconnected with the region of disturbance in Southern America. And it is rather singular that even the earthquakes which have occurred in the valley of the Mississippi seem to be connected with the West Indian and South American volcanic region. The violent earthquakes which took place at New Madrid in 1812, occurred at exactly the same time as the earthquake of Paranas, "so that it is possible," says Sir Charles Lyell, "that these two points are part of one volcanic region."

THE GREATEST SEA-WAVE EVER KNOWN.

On August 13, 1868, one of the most terrible calamities which has ever visited a people befell the unfortunate inhabitants of Peru. In that land earthquakes are nearly as common as rain-storms are with us; and shocks by which whole cities are changed into a heap of ruins are by no means infrequent. Yet even in Peru, "the land of earthquakes," as Humboldt has termed it, no such catastrophe as that of August, 1868, had occurred within the memory of man. It was not one city which was laid in ruins, but a whole empire. Those who perished were counted by tens of thousands, while the property destroyed by the earthquake was valued at millions of pounds sterling.

Although so many months have passed since this terrible calamity took place, scientific men have been busily engaged until quite recently in endeavoring to ascertain the real significance of the various events which were observed during and after the occurrence of the earthquake. The geographers of Germany have taken a special interest in interpreting the evidence afforded by this great manifestation of Nature's powers. Two papers have been written recently on the great earthquake of August 13, 1868, one by Professor von Hochstetter, the other by Herr von Tschudi, which present an interesting account of the various effects, by land and by sea, which resulted from the tremendous upheaving force to which the western flanks of the Peruvian Andes were subjected on that day. The effects on land, although surprising and terrible, yet only differ in degree from those which have been observed in other earthquakes. But the progress of the great sea-wave which was generated by the upheaval of the Peruvian shores and propagated over the whole of the Pacific Ocean differs altogether from any earthquake phenomena before observed. Other earthquakes have indeed been followed by oceanic disturbances; but these have been accompanied by terrestrial motions, so as to suggest the idea that they had been caused by the motion of the sea-bottom, or of the neighboring land. In no instance has it ever before been known that a well-marked wave of enormous proportions should have been propagated over the largest ocean-tract on our globe, by an earth-shock whose direct action was limited to a relatively small region, and that region not situated in the centre, but on one side of the wide area traversed by the wave.

We propose to give a brief sketch of the history of this enormous sea-wave. In the first place, however, it may be well to remind the reader of a few of the more prominent features of the great shock to which this wave owed its origin.

It was at Arequipa, at the foot of the lofty volcanic mountain

Misti, that the most terrible effects of the great earthquake were experienced. Within historic times Misti has poured forth no lava-streams, but that the volcano is not extinct is clearly evidenced by the fact that in 1542 an enormous mass of dust and ashes was vomited forth from its crater. On August 13, 1868, Misti showed no signs of being disturbed. So far as their volcanic neighbor was concerned, the 44,000 inhabitants of Arequipa had no reason to anticipate the catastrophe which presently befell them. At five minutes past five an earthquake-shock was experienced, which, though severe, seems to have worked little mischief. Half a minute later, however, a terrible noise was heard beneath the earth; a second shock more violent than the first was felt; and then began a swaying motion, gradually increasing in intensity. In the course of the first minute this motion had become so violent that the inhabitants ran in terror out of their houses into the streets and squares. In the next two minutes the swaying movement had so increased that the more lightly-built houses were cast to the ground, and the flying people could scarcely keep their feet. "And now," says Von Tschudi, "there followed during two or three minutes a terrible scene. The swaying motion which had hitherto prevailed changed into fierce vertical upheaval. The subterranean roaring increased in the most terrifying manner: then were heard the heart-piercing shrieks of the wretched people, the bursting of walls, the crashing fall of houses and churches, while over all rolled thick clouds of a yellowish-black dust, which, had they been poured forth many minutes longer, would have suffocated thousands." Although the shocks had lasted but a few minutes, the whole town was destroyed. Not one building remained uninjured, and there were few which did not lie in shapeless heaps of ruins.

At Tacna and Arica, the earth-shock was less severe, but strange and terrible phenomena followed it. At the former place a circumstance occurred, the cause and nature of which yet remain a mystery. About three hours after the earthquake — in other words, at about eight o'clock in the evening — an intensely brilliant light made its appearance above the neighboring mountains. It lasted for fully half an hour, and has been ascribed to the eruption of some as yet unknown volcano.

At Arica the sea-wave produced even more destructive effects than had been caused by the earthquake. About twenty minutes after the first earth-shock, the sea was seen to retire, as if about to leave the shores wholly dry; but presently its waters returned with tremendous force. A mighty wave, whose length seemed immeasurable, was seen advancing like a dark wall upon the unfortunate town, a large part of which was overwhelmed by it. Two ships, the Peruvian corvette "America" and the United States "double-ender" "Wateree," were carried nearly half a mile to the north of Arica, beyond the railroad which runs to Tacna, and there left stranded high and dry. This enormous wave was considered by the English vice-consul at Arica to have been fully fifty feet in height.

At Chala, three such waves swept in after the first shocks of earthquake. They overflowed nearly the whole of the town, the sea passing more than half a mile beyond its usual limits.

At Islay and Iquique similar phenomena were manifested. At the former town the sea flowed in no less than five times, and each time with greater force. Afterward the motion gradually diminished, but even an hour and a half after the commencement of this strange disturbance, the waves still ran forty feet above the ordinary level. At Iquique, the people beheld the inrushing wave while it was still a great way off. A dark-blue mass of water, some fifty feet in height, was seen sweeping in upon the town with inconceivable rapidity. An island lying before the harbor was completely submerged by the great wave, which still came rushing on, black with the mud and slime it had swept from the sea-bottom. Those who witnessed its progress from the upper balconies of their houses, and presently saw its black mass rushing close beneath their feet, looked on their safety as a miracle. Many buildings were indeed washed away, and in the low-lying parts of the town there was a terrible loss of life. After passing far inland, the wave slowly returned seaward, and strangely enough, the sea, which elsewhere heaved and tossed for hours after the first great wave had swept over it, here came soon to rest.

At Callao a yet more singular instance was afforded of the effect which circumstances may have upon the motion of the sea after a great earthquake has disturbed it. In former earthquakes Callao has suffered terribly from the effects of the great sea-wave. In fact, on two occasions the whole town has been destroyed, and nearly all its inhabitants have been drowned, through the inrush of precisely such waves as flowed into the ports of Arica and Chala. But upon this occasion the centre of subterranean disturbance must have been so situated that either the wave was diverted from Callao, or more probably two waves reached Callao from different sources and at different times, so that the two undulations partly counteracted each other. Certain it is that, although the water retreated strangely from the coast near Callao, insomuch that a wide tract of the sea-bottom was uncovered, there was no inrushing wave comparable with those described above. The sea afterward rose and fell in an irregular manner, a circumstance confirming the supposition that the disturbance was caused by two distinct oscillations. Six hours after the occurrence of the earth-shock, the double oscillations seemed for a while to have worked themselves into unison, for at this time three considerable waves rolled in upon the town. But clearly these waves must not be compared with those which in other instances had made their appearance within half an hour of the earth-throes. There is little reason to doubt that if the separate oscillations had reënforced each other earlier, Callao would have been completely destroyed. As it was, a considerable amount of mischief was effected; but the motion of the sea presently became irregular again, and so continued until the morning of August 14th, when it began to ebb with some regularity. But during the 14th there were occasional renewals of the irregular motion, and several days elapsed before the regular ebb and flow of the sea were resumed.

Such were among the phenomena presented in the region where the earthquake itself was felt. It will be seen at once that within this region, or rather along that portion of the sea-coast which falls within the central region of disturbance, the true character of the sea-wave generated by the earthquake could not be recognized. If a rock fall from a lofty cliff into a comparatively shallow sea, the water around the place where the rock has fallen is disturbed in an irregular manner. The sea seems at one place to leap up and down; elsewhere one wave seems to beat against another, and the sharpest eye can detect no law in the motion of the seething waters. But presently, outside the scene of disturbance, a circular wave is seen to form, and if the motion of this wave be watched, it is seen to present the most striking contrast with the turmoil and confusion at its centre. It sweeps onward and outward in a regular undulation. Gradually it loses its circular figure (unless the sea-bottom happens to be unusually level), showing that although its motion is everywhere regular, it is not everywhere equally swift. A wave of this sort, though incomparably vaster, swept swiftly away on every side from the scene of the great earthquake near the Peruvian Andes. It has been calculated that the width of this wave varied from one million to five million feet, or roughly from 200 to 1,000 miles, while, when in mid-Pacific, the length of the wave, measured along its summit in a widely-curved path from one side to another of the great ocean, cannot have been less than 8,000 miles.

We cannot tell how deep-seated was the centre of subterranean action; but there can be no doubt it was very deep indeed, because otherwise the shock felt in towns separated from each other by hundreds of miles could not have been so nearly contemporaneous. Therefore the portion of the earth's crust upheaved must have been enormous, for the length of the region where the direct effects of the earthquake were perceived is estimated by Professor von Hochstetter at no less than 240 miles. The breadth of the region is unknown, because the slope of the Andes on one side and the ocean on the other concealed the motion of the earth's crust.

The great ocean-wave swept, as we have said, in all directions around the scene of the earth-throe. Over a large part of its course its passage was unnoted, because in the open sea the effects even of so vast an undulation could not be perceived. A ship would slowly rise as the crest of the great wave passed under her, and then as slowly sink again. This may seem strange, at first sight, when it is remembered that in reality the great sea-wave we are considering swept at the rate of three or four hundred sea-miles an hour over the larger part of the Pacific. But when the true character of ocean-waves is understood, when it is remembered that there is no transference of the water itself at this enormous rate, but simply a transmission of motion (precisely as when in a high wind waves sweep rapidly over a cornfield, while yet each cornstalk remains fixed in the ground), it will be seen that the effects of the great sea-wave could only be perceived near the shore. Even there, as we shall presently see, there was much to convey the impression that the land itself was rising and falling rather than that the deep was moved. But among the hundreds of ships, which were sailing upon the Pacific when its length and breadth were traversed by the great sea-wave, there was not one in which any unusual motion was perceived.

In somewhat less than three hours after the occurrence of the earthquake, the ocean-wave inundated the port of Coquimbo, on the Chilian seaboard, some 800 miles from Arica. An hour or so later it had reached Constitucion, 450 miles farther south; and here for some three hours the sea rose and fell with strange violence. Farther south, along the shore of Chili, even to the island of Chiloe, the shore-wave travelled, though with continually diminishing force, owing, doubtless, to the resistance which the irregularities of the shore opposed to its progress.

The northerly shore-wave seems to have been more considerable; and a moment's study of a chart of the two Americas will show that this circumstance is highly significant. When we remember that the principal effects of the land-shock were experienced within the angle which the Peruvian Andes form with the long north-and-south line of the Chilian and Bolivian Andes, we see at once that had the centre of the subterranean action been near the scene where the most destructive effects were perceived, no sea-wave, or but a small one, could have been sent toward the shores of North America. The projecting shores of northern Peru and Ecuador could not have failed to divert the sea-wave toward the west; and though a reflected wave might have reached California, it would only have been after a considerable interval of time, and with dimensions much less than those of the sea-wave which travelled southward. When we see that, on the contrary, a wave of even greater proportions travelled toward the shores of North America, we seem forced to the conclusion that the centre of the subterranean action must have been so far to the west that the sea-wave generated by it had a free course to the shores of California.

Be this as it may, there can be no doubt that the wave which swept the shores of Southern California, rising upward of sixty feet above the ordinary sea-level, was absolutely the most imposing of all the indirect effects of the great earthquake. When we consider that even in San Pedro Bay, fully five thousand miles from the centre of disturbance, a wave twice the height of an ordinary house rolled in with unspeakable violence only a few hours after the occurrence of the earth-throe, we are most strikingly impressed with the tremendous energy of the earth's movement.

Turning to the open ocean, let us track the great wave on its course past the multitudinous islands which dot the surface of the great Pacific.

The inhabitants of the Sandwich Islands, which lie about 6,300 miles from Arica, might have imagined themselves safe from any effects which could be produced by an earthquake taking place so far away from them. But on the night between August 13th and 14th, the sea around this island-group rose in a surprising manner, insomuch that many thought the islands were sinking, and would shortly subside altogether beneath the waves. Some of the smaller islands, indeed, were for a time completely submerged. Before long, however, the sea fell again, and as it did so the observers "found it impossible to resist the impression that the islands were rising bodily out of the water." For no less than three days this strange oscillation of the sea continued to be experienced, the most remarkable ebbs and floods being noticed at Honolulu, on the island of Woahoo.

But the sea-wave swept onward far beyond these islands.

At Yokohama, in Japan, more than 10,500 miles from Arica, an enormous wave poured in on August 14th, but at what hour we have no satisfactory record. So far as distance is concerned, this wave affords most surprising evidence of the stupendous nature of the disturbance to which the waters of the Pacific Ocean had been subjected. The whole circumference of the earth is but 25,000 miles, so that this wave had travelled over a distance considerably greater than two-fifths of the earth's circumference. A distance which the swiftest of our ships could not traverse in less than six or seven weeks had been swept over by this enormous undulation in the course of a few hours.

More complete details reach us from the Southern Pacific.

Shortly before midnight the Marquesas Isles and the low-lying Tuamotu group were visited by the great wave, and some of these islands were completely submerged by it. The lonely Opara Isle, where the steamers which run between Panama and New Zealand have their coaling-station, was visited at about half-past eleven in the evening by a billow which swept away a portion of the coal-depot. Afterward great waves came rolling in at intervals of about twenty minutes, and several days elapsed before the sea resumed its ordinary ebb and flow.

It was not until about half-past two on the morning of August 14th, that the Samoa Isles (sometimes called the Navigator Islands) were visited by the great wave. The watchmen startled the inhabitants from their sleep by the cry that the sea was about to overwhelm them; and already, when the terrified people rushed from their houses, the sea was found to have risen far above the highest water-mark. But it presently began to sink again, and then commenced a series of oscillations, which lasted for several days, and were of a very remarkable nature. Once in every quarter of an hour the sea rose and fell, but it was noticed that it rose twice as rapidly as it sank. This peculiarity is well worth remarking. The eminent physicist Mallet speaks thus (we follow Lyell's quotation) about the waves which traverse an open sea: "The great sea-wave, advancing at the rate of several miles in a minute, consists, in the deep ocean, of a long, low swell of enormous volume, having an equal slope before and behind, and that so gentle that it might pass under a ship without being noticed. But when it reaches the edge of soundings, its front slope becomes short and steep, while its rear slope is long and gentle." On the shores visited by such a wave, the sea would appear to rise more rapidly than it sank. We have seen that this happened on the shores of the Samoa group, and therefore the way in which the sea rose and fell on the days following the great earthquake gave significant evidence of the nature of the sea-bottom in the neighborhood of these islands. As the change of the great wave's figure could not have been quickly communicated, we may conclude with certainty that the Samoan Islands are the summits of lofty mountains, whose sloping sides extend far toward the east.

This conclusion affords interesting evidence of the necessity of observing even the seemingly trifling details of important phenomena.

The wave which visited the New-Zealand Isles was altogether different in character, affording a noteworthy illustration of another remark of Mallet's. He says that where the sea-bottom slopes in such a way that there is water of some depth close in-shore, the great wave may roll in and do little damage; and we have seen that so it happened in the case of the Samoan Islands. But he adds, that "where the shore is shelving, there will be first a retreat of the water, and then the wave will break upon the beach and roll far in upon the land." This is precisely what happened when the great wave reached the eastern shores of New Zealand, which are known to shelve down to very shallow water continuing far away to sea toward the east.

At about half-past three on the morning of August 14th, the water began to retreat in a singular manner from the port of Littleton, on the eastern shores of the southernmost of the New Zealand Islands. At length the whole port was left entirely dry, and so remained for about twenty minutes. Then the water was seen returning like a wall of foam ten or twelve feet in height, which rushed with a tremendous noise upon the port and town. Toward five o'clock the water again retired, very slowly as before, not reaching its lowest ebb until six. An hour later, a second huge wave inundated the port. Four times the sea retired and returned with great power at intervals of about two hours. Afterward the oscillation of the water was less considerable, but it had not wholly ceased until August 17th, and only on the 18th did the regular ebb and flow of the tide recommence.

Around the Samoa group the water rose and fell once in every fifteen minutes, while on the shores of New Zealand each oscillation lasted no less than two hours. Doubtless the different depths of water, the irregular conformation of the island-groups, and other like circumstances, were principally concerned in producing these singular variations. Yet they do not seem fully sufficient to account for so wide a range of difference. Possibly a cause yet unnoticed may have had something to do with the peculiarity. In waves of such enormous extent, it would be quite impossible to determine whether the course of the wave-motion was directed full upon a line of shore or more or less obliquely. It is clear that in the former case the waves would seem to follow each other more swiftly than in the latter, even though there were no difference in their velocity.

Far on beyond the shores of New Zealand the great wave coursed, reaching at length the coast of Australia. At dawn of August 14th, Moreton Bay was visited by five well-marked waves. At Newcastle, on the Hunter River, the sea rose and fell several times in a remarkable manner, the oscillatory motion commencing at half-past six in the morning. But the most significant evidence of the extent to which the sea-wave travelled in this direction was afforded at Port Fairy, Belfast, South Victoria. Here the oscillation of the water was distinctly perceived at mid-day on August 14th; and yet, to reach this point, the sea-wave must not only have travelled on a circuitous course nearly equal in length to half the circumference of the earth, but must have passed through Bass's Straits, between Australia and Van Diemen's Land, and so have lost a considerable portion of its force and dimensions. When we remember that had not the effects of the earth-shock on the water been limited by the shores of South America, a wave of disturbance equal in extent to that which travelled westward would have swept toward the east, we see that the force of the shock was sufficient to have disturbed the waters of an ocean covering the whole surface of the earth. For the sea-waves which reached Yokohama in one direction and Port Fairy in another had each traversed a distance nearly equal to half the earth's circumference; so that if the surface of the earth were all sea, waves setting out in opposite directions from the centre of disturbance would have met each other at the antipodes of their starting-point.

It is impossible to contemplate the effects which followed the great earthquake — the passage of a sea-wave of enormous volume over fully one-third of the earth's surface, and the force with which, on the farthermost limits of its range, the wave rolled in upon shores more than 10,000 miles from its starting-place — without feeling that those geologists are right who deny that the subterranean forces of the earth are diminishing in intensity. It may be difficult, perhaps, to look on the effects which are ascribed to ancient earth-throes without imagining for a while that the power of modern earthquakes is altogether less. But when we consider fairly the share which time had in those ancient processes of change, when we see that while mountain-ranges were being upheaved or valleys depressed to their present position, race after race and type after type appeared on the earth, and lived out the long lives which belong to races and to types, we are recalled to the remembrance of the great work which the earth's subterranean forces are still engaged upon. Even now, continents are being slowly depressed or upheaved; even now mountain-ranges are being raised to a new level, table-lands are in process of formation, and great valleys are being gradually scooped out. It may need an occasional outburst such as the earthquake of August, 1868, to remind us that great forces are at work beneath the earth's surface. But, in reality, the signs of change have long been noted. Old shore-lines shift their place, old soundings vary; the sea advances in one place and retires in another; on every side Nature's plastic hand is at work modelling and remodelling the earth, in order that it may always be a fit abode for those who are to dwell upon it.

THE USEFULNESS OF EARTHQUAKES.

We have lately had fearful evidence of the energy of the earth's internal forces. A vibration which, when considered with reference to the dimensions of the earth's globe, may be spoken of as an indefinitely minute quivering limited to an insignificant area, has sufficed to destroy the cities and villages of whole provinces, to cause the death of thousands of human beings, and to effect a destruction of property which must be estimated by millions of pounds sterling. Such a catastrophe as this serves indeed to show how poor and weak a creature man is in presence of the grand workings of Nature. The mere throes which accompany her unseen subterranean efforts suffice to crumble man's strongest buildings in a moment into dust, while the unfortunate inhabitants are either crushed to death among the ruins, or forced to remain shuddering spectators of the destruction of their homes.

At first sight it may seem paradoxical to assert that earthquakes, fearfully destructive as they have so often proved, are yet essentially preservative and restorative phenomena; yet this is strictly the case. Had no earthquakes taken place in old times, man would not now be living on the face of the earth; if no earthquakes were to take place in future, the term of man's existence would be limited within a range of time far less than that to which it seems likely, in all probability, to be extended.

If the solid substance of the earth formed a perfect sphere in ante-geologic times — that is, in ages preceding those to which our present geologic studies extend — there can be no doubt that there was then no visible land above the surface of the water; the ocean must have formed a uniformly deep covering to the submerged surface of the solid globe. In this state of things, nothing but the earth's subterranean forces could tend to the production of continents and islands. Let us be understood. We are not referring to the possibility or impossibility that lands and seas should suddenly have assumed their present figure without convulsion of any sort; this *might* have happened, since the Creator of all things can doubtless modify all things according to His will; we merely say that, assuming that in the beginning, as now, He permitted all things to work according to the laws He has appointed, then, undoubtedly, the submerged earth must have risen above the sea by the action of those very forms of force which produce the earthquake in our own times.

However this may be, it is quite certain that when once continents and islands had been formed, there immediately began a struggle between destructive and restorative (rather, perhaps, than preservative) forces.

The great enemy of the land is water, and water works the destruction of the land in two principal ways.

In the first place, the sea tends to destroy the land by beating on its shores, and thus continually washing it away. It may seem at first sight that this process must necessarily be a slow one; in fact, many may be disposed to say that it is certainly a slow process, since we see that it does not alter the forms of continents and islands perceptibly in long intervals of time. But, as a matter of fact, we have never had an opportunity of estimating the full effects of this cause, since its action is continually being checked by the restorative forces we shall presently have to consider. Were it not thus checked, there can be little doubt that its effects would be cumulative; for the longer the process continued — that is, the more the land was beaten away — the higher would the sea rise, and the greater power would it have to effect the destruction of the remaining land.

We proceed to give a few instances of the sea's power of effecting the rapid destruction of the land when nothing happens to interfere with the local action — premising, that this effect is altogether insignificant in comparison with that which would take place, even in that particular spot, if the sea's action were *everywhere* left unchecked.

The Shetland Isles are composed of substances which seem, of all others, best fitted to resist the disintegrating forces of the sea — namely, granite, gneiss, mica-slate, serpentine, greenstone, and many other forms of rock; yet, exposed as these islands are to the uncontrolled violence of the Atlantic Ocean they are undergoing a process of destruction which, even within historical times, has produced very noteworthy changes. "Steep cliffs are hollowed out," says Sir Charles Lyell, "into deep caves and lofty arches; and almost every promontory ends in a cluster of rocks, imitating the forms of columns, pinnacles, and obelisks." Speaking of one of the islands of this group, Dr. Hibbert says: "The isle of Stenness presents a scene of unequalled desolation. In stormy winters, large blocks of stone are overturned, or are removed from their native beds, and hurried to a distance almost incredible. In the winter of 1802, a tabular mass, eight feet two inches by seven feet, and five feet one inch thick, was dislodged from its bed, and carried to a distance of from eighty to ninety feet." In other parts of the Shetland Isles, where the sea has encountered less solid materials, the work of destruction has proceeded yet more effectively. In Roeness, for example, the sea has wrought its way so fiercely, that a large cavernous aperture 250 feet long has been hollowed out. "But the most sublime scene," says Dr. Hibbert, "is where a mural pile of porphyry, escaping the process of disintegration that is devastating the coast, appears to have been left as a sort of rampart against the inroads of the ocean. The Atlantic, when provoked by wintry gales, batters against it with all the force of real artillery; and the waves, in their repeated assaults, have at length forced for themselves an entrance. This breach, named the Grind of the Navir, is widened every winter by the overwhelming surge that, finding a passage through it, separates large stones from its sides, and forces them to a distance of no less than 180 feet. In two or three spots, the fragments which have been detached are brought together in immense heaps, that appear as an accumulation of cubical masses, the product of some quarry."

Let us next turn to a portion of the coast-line of Great Britain which is neither defended, on the one hand, by barriers of rock, nor attacked, on the other, by the full fury of the Atlantic currents. Along the whole coast of Yorkshire, we find evidences of a continual process of dilapidation. Between the projecting headland of Flamborough and Spurn Point (the coast of Holderness), the waste is particularly rapid. Many spots, which are now mere sand-banks, are marked in the old maps of Yorkshire as the sites of ancient towns and villages. Speaking of Hyde (one of these), Pennant says: "Only the tradition is left of this town." Owthorne and its church have been for the most part destroyed, as also Auburn, Hartburn, and Kilnsea. Mr. Phillips, in his "Geology of Yorkshire," states that not unreasonable fears are entertained that, at some future time, Spurn Point itself will become an island, or be wholly washed away, and then the ocean, entering into the estuary of the Humber, will cause great devastation. Pennant states that "several places, once towns of note upon the Humber, are now only recorded in history; and Ravensperg was at one time a rival of Hull, and a port so very considerable in 1342, that Edward Baliol and the confederated English barons sailed from hence to invade Scotland; and Henry IV., in 1399, made choice of this port to land at, to effect the deposal of Richard II.; yet the whole of this has since been devoured by the merciless ocean; extensive sands, dry at low water, are to be seen in their stead." The same writer also describes Spurn Point as shaped like a sickle, and the land to the north, he says, was "perpetually preyed on by the fury of the German Sea, which devours whole acres at a time."

The decay of the shores of Norfolk and Suffolk is also remarkably rapid. Sir Charles Lyell relates some facts which throw an interesting light on the ravages which the sea commits upon the land here. It was computed that when a certain inn was built at Sherringham, seventy years would pass before the sea could reach the spot; "the mean loss of land being calculated from previous observations to be somewhat less than one yard annually." But no allowance had been made for the fact that the ground sloped *from* the sea. In consequence of this peculiarity, the waste became greater and greater every year as the cliff grew lower. "Between the years 1824 and 1829, no less than seventeen yards were swept away;" and when Sir Charles Lyell saw the place, only a small garden was left between the building and the sea. We need hardly add that all vestiges of the inn have long since been swept away. Lyell also relates that, in 1829, there was a depth of water sufficient to float a frigate at a point where, less than half a century before, there stood a cliff fifty feet high with houses upon it.

We have selected these portions of the coast of Great Britain, not because the destruction of our shores is greater here than elsewhere, but as serving to illustrate processes of waste and demolition which are going on around all the shores, not merely of Great Britain, but of every country on the face of the earth. Here and there, as we have said, there are instances in which a contrary process seems to be in action. Low-lying banks and shoals are formed — sometimes along stretches of coast extending for a considerable distance. But when we consider these formations closely, we find that they rather afford evidence of the energy of the destructive forces to which the land is subject than promise to make up for the land which has been swept away. In the first place, every part of these banks consists of the *débris* of other coasts. Now, we cannot doubt that of earth which is washed away from our shores, by far the larger part finds its way to the bottom of the deep seas; a small proportion only can be brought (by some peculiarity in the distribution of ocean-currents, or in the progress of the tidal wave) to aid in the formation of shoals and banks. The larger, therefore, such shoals and banks may be, the larger must be the amount of land which has been washed away never to reappear. And although banks and shoals of this sort grow year by year larger and larger, yet (unless added to artificially) they continue always either beneath the surface of the water, in the case of shoals, or but very slightly raised above the surface. Now, if we suppose the destruction of land to proceed unchecked, it is manifest that at some period, however remote, the formation of shoals and banks must come to an end, owing to the continual diminution of the land from the demolition of which they derive their substance. In the meantime, the bed of the sea would be continually filling up, the level of the sea would be continually rising, and thus the banks would either be wholly submerged through the effect of this cause alone, or they would have so slight an elevation above the sea-level, that they would offer little resistance to the destructive effects of the sea, which would now have no other land to act upon.

But we have yet to consider the second principal cause of the wasting away of the land. The cause we have just been dealing with acts upon the shores or outlines of islands and continents; the one we have now to consider acts upon their interior. It will, perhaps, hardly be supposed that the fall of rain upon the land could have any appreciable influence in the demolition of continents; but as a matter of fact, there are few causes to which geologists are disposed to ascribe more importance. The very fact that enormous deltas have been formed at the mouths of many rivers — in other words, the actual growth of continents through the effects of rainfall — is a proof how largely this cause must tend to destroy and disintegrate the interiors of our continents. Dwelling on this point, Sir Charles Lyell presents the following remarkable illustration: "During a tour in Spain," he writes, "I was surprised to see a district of gently-undulating ground in Catalonia, consisting of red and gray sandstone, and in some parts of red marl, almost entirely denuded of herbage; while the roots of the pines, holm oaks, and some other trees, were half exposed, as if the soil had been washed away by a flood. Such is the state of the forests, for example, between Oristo and Vich, and near San Lorenzo. But, being overtaken by a violent thunderstorm in the month of August, I saw the whole surface, even the highest levels of some flat-topped hills, streaming with mud, while on every declivity the devastation of torrents was terrific. The peculiarities in the physiognomy of the district were at once explained; and I was taught that, in speculating on the greater effects which the direct action of rain may once have produced on the surface of certain parts of England, we need not revert to periods when the heat of the climate was tropical."

Combining the effects of the sea's action upon the shores of continents, and of the action of rain upon their interior, and remembering that unless the process of demolition were checked in some way, each cause would act from year to year with new force — one through the effects of the gradual rise of the sea-bed, and the other through the effects of the gradual increase of the surface of ocean exposed to the vaporizing action of the sun, which increase would necessarily increase the quantity of rain yearly precipitated on the land — we see the justice of the opinion expressed by Sir John Herschel, that, "had the primeval world been constructed as it now exists, time enough has elapsed, and force enough directed to that end has been in activity, *to have long ago destroyed every vestige of land.*"

We see, then, the necessity that exists for the action of some restorative or preservative force sufficient to counteract the effects of the continuous processes of destruction we have indicated above. If we consider, we shall see that the destructive forces owe their efficiency to their levelling action, that is, to their influence in reducing the solid part of the earth to the figure of a perfect sphere; therefore the form of force which is required to counteract them is one that shall tend to produce irregularities in the surface-contour of the earth. And it will be remarked that, although *upheaval* is the process which appears at first sight to be the only effectual remedy to the levelling action of rains and ocean-currents, yet the forcible depression of the earth's surface may prove in many instances yet more effective, since it may serve to reduce the sea-level in other places.

Now, the earth's subterranean forces serve to produce the very effects which are required in order to counteract the continual distintegration of the shores and interior parts of continents. In the first place, their action is not distributed with any approach to uniformity over different parts of the earth's crust, and therefore the figure they tend to give to the surface of that crust is not that of a perfect sphere. This, of itself, secures the uprising of some parts of the solid earth above the sea-level. But this is not all. On a comparison of the various effects due to the action of subterranean forces, it has been found that the forces of upheaval act (on the whole) more powerfully under continents, and especially under the shore-lines of continents, while the forces of depression act most powerfully (on the whole) under the bed of the ocean. It need hardly be said that whenever the earth is upheaved in one part, it must be depressed somewhere else. Not necessarily at the same instant, it should be remarked. The process of upheaval may be either momentarily accompanied by a corresponding process of depression, or the latter process may take place by a gradual action of the elastic powers of the earth's crust; but in one way or the other, the balance between upheaval and depression must be restored. Hence, if it can be shown that for the most part the forces of upheaval act underneath the land, it follows — though we may not be able to recognize the fact by obvious visible signs — that processes of depression are taking place underneath the ocean. Now, active volcanoes mark the centre of a district of upheaval, and nearly all volcanoes are found near the sea. It seems as if Nature had provided against the inroads of the ocean by seating the earth's upheaving forces just where they are most wanted.

Even in earthquake districts which have no active vent, the same law is found to prevail. It is supposed by the most eminent seismologists that earthquake regions around a volcano, and earthquake regions apparently disconnected from any outlet, differ only in this respect, that in the one case the subterranean forces have had sufficient power to produce the phenomena of eruption, while in the other they have not. In "earthquakes," says Humboldt, "we have evidence of a volcano-producing force; but such a force, as universally diffused as the internal heat of the globe, and proclaiming itself everywhere, rarely acts with sufficient energy to produce actual eruptive phenomena; and when it does so, it is only in isolated and particular places."

Of the influence of the earth's subterranean forces in altering the level of the land, we might quote many remarkable instances, but considerations of space compel us to confine ourselves to two or three. The slow processes of upheaval or depression may, perhaps, seem less immediately referable to subterranean action than those which are produced during the progress of an actual earthquake. We pass over, therefore, such phenomena as the gradual uprising of Sweden, the slow sinking of Greenland, and (still proceeding westward) the gradual uprising of Nova Scotia and the shores of Hudson's Bay. Remarkable and suggestive as these phenomena really are, and indisputable as the evidence is on which they rest, they will probably seem much less striking to our readers than those which we are now about to quote.

On the 19th of November, 1822, a widely-felt and destructive earthquake was experienced in Chili. On the next day, it was noticed for the first time that a broad line of sea-coast had been deserted by the sea for more than one hundred miles. A large part of this tract was covered by shell-fish, which soon died, and exhaled the most offensive effluvia. Between the old low-water mark and the new one, the fishermen found burrowing shells, which they formerly had to search for amid the surf. Rocks some way out to sea which had formerly been covered, were now dry at half ebb-tide.

Careful measurements showed that the rise of the land was greater at some distance inshore than along the beach. The water-course of a mill about a mile inland from the sea had gained a fall of fourteen inches in little more than a hundred yards. At Valparaiso, the rise was three feet; at Quintero, four feet.

In February, 1835, and in November, 1837, a large tract of Chili was similarly shaken, a permanent rise of two feet following the former earthquake, and a rise of eight feet the latter.

The earthquake which took place at Cutch in 1819 is perhaps in some respects yet more remarkable. In this instance, phenomena of subsidence, as well as phenomena of upheaval, were witnessed. The estuary of the Indus, which had long been closed to navigation — being, in fact, only a foot deep at ebb-tide, and never more than six feet at flood — was deepened in parts to more than eighteen feet at low water. The fort and village of Sindree was submerged, only the tops of houses and walls being visible above the water. But although this earthquake seemed thus to have a land-destroying instead of a land-creating effect, yet the instances of upheaval were, even in this case, far more remarkable than those of depression. "Immediately after the shock," says Sir Charles Lyell, "the inhabitants of Sindree saw at a distance of five miles and a half from their village, a long elevated mound, where previously there had been a low and perfectly level plain. To this uplifted tract they gave the name of Ullah-Bund, or the 'Mound of God,' to distinguish it from several artificial dams previously thrown across the eastern arm of the Indus. It has been ascertained," he adds, "that this new-raised country is upward of fifty miles in length from east to west, running parallel to the line of subsidence which caused the grounds around Sindree to be flooded. The breadth of the elevation is conjectured to be in some parts sixteen miles, and its greatest ascertained height above the original level of the delta is ten feet — an elevation which appears to the eye to be very uniform throughout."

THE FORCING POWER OF RAIN.

THERE is an old proverb which implies that England need never fear drought; and we have had clear evidence this year that an exceptionally dry summer is not necessarily followed by a bad harvest. But we believe that when a balance is carefully struck between the good and the evil effects resulting from excessive drought in England, it will be found that the latter largely prevail. In fact, it is only necessary to observe the effects which have followed the recent wet weather to recognize the fact that rain has a forcing power, the very diminished supply of which at the due season cannot fail to have seriously injurious effects. In various parts of England we see evidences of the action of such a power during the present autumn in the blossoming of trees, in the flowering of primroses and other spring plants, in rich growths of fungi, and in various other ways. It cannot be doubted that there is here a comparative waste of powers, which, expended in due season, would have produced valuable results.

The modern theories of the correlation of force suffice to show how enormous a loss a country suffers when there is a failure in the supply of rain, or when that supply comes out of its due season. When we consider rain in connection with the causes to which it is due, we begin to recognize the enormous amount of power of which the ordinary rainfall of a country is the representative; and we can well understand how it is that "the clouds drop fatness on the earth."

The sun's heat is, of course, the main agent — we may almost say the only agent — in supplying the rainfall of a country.

The process of evaporation carried on over large portions of the ocean's surface is continually storing up enormous masses of water in the form of invisible aqueous vapor, ready to be transformed into cloud, then wafted for hundreds of miles across seas and continents, to be finally precipitated over this or that country, according to the conditions which determine the downfall of rain. These processes do not appear, at first sight, indicative of any very great expenditure of force, yet, in reality, the force-equivalent of the rain-supply of England alone for a single year is something positively startling. It has been calculated that the amount of heat required to evaporate a quantity of water which would cover an area of 100 miles to a depth of one inch would be equal to the heat which would be produced by the combustion of half a million tons of coals. The amount of force of which this consumption of heat would be the equivalent, corresponds to that which would be required to raise a weight of upward of one thousand millions of tons to a height of one mile. Now, when we remember that the area of Great Britain and Ireland is about 120,000 square miles, and that the annual rainfall averages about 25 inches, we see that the force-equivalent of the rainfall is enormous. All the coal which could be raised from our English coal-mines in thousands of years would not give out heat enough to produce England's rain-supply for a single year. When to this consideration we add the circumstance that the force of rain produces bad as well as good effects — the former when the rain falls at undue seasons or in an irregular manner, the latter only when the rainfall is distributed in the usual manner among the seasons — we see that an important loss accrues to a country in such exceptional years as the present.

There are few subjects more interesting than those depending on the correlation of physical forces; and we may add that there are few the study of which bears more largely on questions of agricultural and commercial economy. It is only of late years that the silent forces of Nature — forces continually in action, but which are too apt to pass unnoticed and unrecognized — have taken their due place in scientific inquiry. Strangely enough, the subject has been found to have at once a most practical bearing on business relations, and an aspect more strikingly poetical than any other subject, perhaps, which men of science have ever taken in hand to investigate. We see the ordinary processes of Nature, as they are termed, taking their place in the workshop of modern wealth, and at the same time exhibited in a hundred striking and interesting physical relations. What, for instance, can be stranger or more poetical than the contrast which Professor Tyndall has instituted between that old friend to the agriculturist — the wintry snowflake — and the wild scenery of the Alps? "I have seen," he says, "the wild stone-avalanches of the Alps, which smoke and thunder down the declivities with a vehemence almost sufficient to stun the observer. I have also seen snow-flakes descending so softly as not to hurt the fragile spangles of which they were composed; yet to produce from aqueous vapor a quantity which a child could carry of that tender material demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanche I have ever seen, and pitch them to twice the height from which they fell."

We may point out in this place the important connection which exists between the rainfall of a country and the amount of forest-land. We notice that in parts of America attention is being paid — with markedly good results — to the influence of forests in encouraging rainfall. We have here an instance in which cause and effect are interchangeable. Rain encourages the growth of an abundant vegetation, and abundant vegetation in turn aids to produce a state of the super-incumbent atmosphere which encourages the precipitation of rain. The consequence is, that it is very necessary to check, before it is too late, the processes which lead to the gradual destruction of forests. If these processes are continued until the climate has become excessively dry, it is almost impossible to remedy the mischief, simply because the want of moisture is destructive to the trees which may be planted to encourage rainfalls. Thus, there are few processes more difficult (as has been found by experience in parts of Spain and elsewhere) than the change of an arid region into a vegetation-covered district. In fact, if the region is one of great extent, the attempt to effect such a change is a perfectly hopeless one. On the other hand, the contrary process — that is, the attempt to change a climate which is too moist into one of less humidity — is in general not attended with much difficulty. A judicious system of clearing nearly always leads to the desired result.

The dryness of the past year has not been due to the want of moisture in the air, nor to the exceptionally unclouded condition of our skies. We believe that, on the whole, the skies have been rather more cloudy than usual this year. The fact that so little dew has fallen is a sufficient proof that the nights have been on the whole more cloudy than usual, since, as is well known, the presence of clouds, by checking the radiation of the earth's heat, prevents (or at least diminishes) the formation of dew. The fact would seem to be that the westerly and south-westerly winds which usually blow over England during a considerable part of the year, bringing with them large quantities of aqueous vapor from above the great Gulf Stream, have this year blown somewhat higher than usual. Why this should be it is not very easy to say. The height of the vapor-laden winds is usually supposed to depend on the heat of the weather. In summer, for instance, the clouds range higher, and therefore travel farther inland before they fall in rain. In winter, on the contrary, they travel lower, and hence the rain falls more freely in the western than in the eastern countries during winter. A similar relation prevails in the Scandinavian peninsula — Norway receiving more rain in winter than in summer, while Sweden receives more rain in summer than in the winter. But this summer the rain-clouds have blown so much higher than usual as to pass beyond England altogether. Possibly we may find an explanation in the fact that before reaching our shores at all the clouds were relieved by heavy rainfalls — probably due to some exceptional electrical relations — over parts of the Atlantic Ocean. It is stated that the steamships from America this summer were, in many instances, drenched by heavy showers until they neared the coasts of England.

A SHOWER OF SNOW-CRYSTALS.

Yesterday morning a remarkably fine fall of snow-stars took place over many parts of London. The crystals were larger and more perfectly formed than is commonly the case in our latitudes, where the conditions requisite for the formation of these beautiful objects are less perfectly fulfilled than in more northerly regions. Many forms were to be noticed which the researches of Scoresby, Glaisher, and Lowe, have shown to be somewhat uncommon.

Many of our readers will, perhaps, be surprised to learn that no less than 1,000 different kinds of snow-crystals have been noticed by the observers named above, and that a large proportion of them have been figured and described. The patterns are of wonderful beauty. A strange circumstance connected with these objects is the fact that for the most part they are found, on a close examination, to be formed of minute colored crystals — some red, some green, others blue or purple. In fact, all the colors of the rainbow are to be seen in the delicate tracery of these fine hexagonal stars. So that in the perfect whiteness of the driven snow we have an illustration of the well-known fact that the colors of the rainbow combine to form the purest white. For the common snow-flake is formed of a large number of such tiny crystals as were falling yesterday; though their beauty is destroyed in the snow-flake, through the effects of collision and partial melting. It may not be very commonly known that ordinary ice, also, is composed of a combination of crystals presenting all the regularity of formation seen in the snow-crystals. This would scarcely be believed by any one who examined a rough mass of ice taken from the surface of a frozen lake. Yet, if a slice be cut from the mass and placed in the sun's light, or before a fire, the beautiful phenomena called ice-flowers make their appearance —" A fairy seems to have breathed upon the ice, and caused transparent flowers of exquisite beauty suddenly to blossom in myriads within it."

When we remember that the enormous ice-bergs of the Arctic and Antarctic seas, the snow-caps which crown the Alps, and Andes, and Himalayas, and the glaciers which urge their way with resistless force down the mountain valleys, are all made up of these delicate and beautiful snow-flowers, we are struck with the force of the strange contrasts which Nature presents to our contemplation. We may say of the snow-crystals what Tennyson said of the small sea-shell. Each snow-star is

"Frail, but a work divine,
Made so fairly well,
So exquisitely minute,
A miracle of design."

Yet — massed together with all the prodigality of Nature's unsparing hand — they crown the everlasting hills; or, falling in avalanche and glacier, overwhelm the stoutest works of man; or, in vast islands of floating ice, show themselves to be

> "Of force to withstand, year upon year, the shock
> Of cataract seas that snap the three-decker's oaken spine."

LONG SHOTS.

Our artillerists have paid more attention of late years to the destructive properties of various forms of cannon than to the question of range. It was different when first the rifling of cannon was under discussion. Then the subject which was most attentively considered (after accuracy of fire) was the range which might possibly be obtained by various improvements in the structure of rifled cannon. Many of our readers will remember how, soon after the construction of Armstrong guns had been commenced in the Government factories, a story was spread abroad of the wonderful practice which had been made with this gun at a range of seven miles. At that tremendous range, a shot had been fired into the middle of a flock of geese, according to one version of the story; but this was presently improved upon, and we were told that a bird had been singled out of the flock by the artillerists and successfully "potted!" Many believed this little narrative; though some few, influenced perhaps by the consideration that a flock of geese would not be visible at a distance of seven miles, were obstinately incredulous. Presently it turned out that the Armstrong gun was incapable of throwing a shot to a distance of seven miles; so that a certain air of improbability has since attached to the narrative. Still there were not wanting those who referred to "Queen Anne's pocket-pistol"— the cannon which was able to throw shot across the Straits of Dover; and in the fulness of their faith in that mythical piece of ordnance, they refused to believe that the skill of modern artillerists was unequal to the construction of cannon even more effective.

If there are any who still believe in the powers ascribed to the far-famed "pocket-pistol," they will find their confidence in modern artillery largely shaken by the announcement that it is considered a great matter that one of Whitworth's cannon should have thrown a shot to a distance of very nearly six and a half miles. Not only is this so, however, but it is well known that no piece of ordnance has ever flung a projectile to so great a distance since first fire-arms were invented; and it may be safely predicted that men will never be able to construct a cannon which — as far as range is concerned — will do much better than this one of Mr. Whitworth's. The greatest range which had ever before been obtained fell somewhat short of six miles. The 7-inch steel gun contrived by Mr. Lynall Thomas had flung a projectile weighing one hundred and seventy-five pounds to a distanec of ten thousand and seventy-five yards; and, according to General Lefroy's "Hand-book of Artillery," that was the greatest range ever recorded. But Mr. Whitworth's cannon throws a shot more than a thousand yards farther.

Very few have any idea of the difficulties which oppose themselves to the attainment of a great range in artillery practice. It may seem, at first sight, the simplest possible matter to obtain an increase of range. Let the gun be made but strong enough to bear a sufficient charge, and range seems to be merely a question of the quantity of powder made use of. But in reality the matter is much more complicated. The artillerist has to contrive that the whole of the powder made use of shall be burned before the shot leaves the cannon, and yet that the charge shall not explode so rapidly as to burst the cannon. If he used some forms of powder, very useful for special purposes, half the charge would be blown out without doing its share of work. On the other hand, there are some combustibles — as gun-cotton and the nitrates — which burn so fast that the gun would be likely to burst before the shot could be expelled. Then, again, the shot must fit so closely that there shall be no windage, and yet not so closely as to resist too much the action of the exploding powder. Again, there is the form of the shot to be considered. A sphere is not the solid which passes most readily through a resisting medium like the air; and yet, other projectiles, which are best so long as they maintain a certain position, meet with a greater resistance when once they begin to move unsteadily. The conoid used in ordinary rifle-practice, for example, passes much more freely through the air, point first, than an ordinary spherical bullet; but if the point did not travel first, as would happen but for the rifling, or even if the conoidal bullet "swayed about" on its course, it would meet with more resistance than a spherical bullet. Hence the question of "fast or slow rifling" has to be considered. "Fast rifling" gives the greater spin, but causes more resistance in the exit of the shot from the barrel; with "slow rifling," these conditions are reversed.

And then the common notion is that a cannon-ball travels in the curve called a parabola, and that artillerists have nothing to do but to calculate all about this parabola, and to deduce the range from the initial velocity according to some simple principles deduced from the properties of the curve. All this is founded on a complete misapprehension of the true difficulties in the way of the problem. Only projectiles thrown with small velocity from the earth travel in parabolic paths. A cannon-ball follows a wholly different kind of curve. The resistance of the air, which seems to most persons a wholly insignificant item in the inquiry, is so enormous in the case of a cannon-ball as to become by far the most important difficulty in the way of the practical artillerist. When a 250-pound shot is hurled with such force from a gun as to cover a range of six miles, the resistance of the air is about forty times the weight of the ball — that is, is equivalent to a weight of upward of four tons. The range in such a case as this is but a small fraction of that which would be given by the ordinary parabolic theory.

As regards artillery practice in war, there are other difficulties in the attainment of a very extended range. Cannon meant for battering down forts cannot possibly be used in the same way that Whitworth's was used at Shoeburyness. If the shot flung from this gun at an elevation of thirty-three degrees could have been watched, it would have been found that it fell to the earth at a much greater angle — that is, much more nearly in a perpendicular direction. On the ordinary parabolic theory, of course, the angle of fall would be the same as the angle of elevation, but under actual circumstances there is an important difference. If forts are to be battered down, however, it will not serve that they should be struck from above; our artillerists must perforce keep to the old method of pounding away at the face of the forts they attack. Therefore, an elevation which is all very well for mortars — that is, when the question merely is of flinging a bomb into a town or fortress — is utterly unsuited for ordinary artillery. With an elevation of 10°, Whitworth's cannon scarcely projected the 250-lb. shot to a distance of three miles.

The progress of the modern science of gunnery certainly tends to increase the distance at which armies will engage each other. With field artillery flinging shot to a distance of two or three miles, and riflemen able to make tolerably sure practice at a distance of three-quarters of a mile, we are not likely often to hear of hand-to-hand conflicts in future warfare. The use of breech-loaders will also tend to the same effect. Hitherto we have scarcely had experience of the results which these changes are to produce on modern warfare. At Sadowa breech-loaders did not encounter breech-loaders, and it was easy for the victors in that battle to come to close quarters with their enemies. But in a battle where both sides are armed with breech-loaders, we shall probably see another sort of affair altogether. The bayonet will be an almost useless addition to the soldier's arms; a charge of cavalry upon well-armed infantry will be almost as hopeless as the famous Balaklava charge; and the artillery on either side will have to play a game at long shots. We venture to anticipate that the first great European war will introduce a total change into the whole system of warlike manœuvres.*

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* The reader need hardly be reminded of the most complete fulfilment of this anticipation.

INFLUENCE OF MARRIAGE ON THE DEATH-RATE.

The Royal Commission on the Law of Marriage has attracted attention to many singular and instructive results of modern statistical inquiry. Not the least important of these is the apparent influence of marriage on the death-rate. For several years it has been noticed by statisticians that the death-rate of unmarried men is considerably higher than the death-rate of married men and widowers. We believe that Dr. Stark, Registrar-General for Scotland, was one of the first to call attention to this peculiarity, as evidenced by the results of two years' returns for Scotland. But the law has since been confirmed by a far wider range of statitical inquiry. The relative proportion between the death-rates of the married and of the unmarried is not absolutely uniform in different countries, but it is fairly enough represented by the following table, which exhibits the mortality per thousand of married and unmarried men in Scotland:

Ages.	Husbands and Widowers.	Unmarried.
20 to 25	6.26	12.31
25 to 30	8.23	14.94
30 to 35	8.65	15.94
35 to 40	11.67	16.02
40 to 45	14.07	18.35
45 to 50	17.04	21.18
50 to 55	19.54	26.34
55 to 60	26.14	28.54
60 to 65	35.63	44.54
65 to 70	52.93	60.21
70 to 75	81.56	102,71
75 to 80	117.85	143.94
80 to 85	173.88	195.40

From this table we are to understand that out of one hundred thousand married persons (including widowers) from 20 to 25 years old, 626 die in the course of each year; while out of a similar number of unmarried persons, between the same ages, no less than 1,231 die in each year. And in like manner all the other lines of the table are to be interpreted.

Commenting on the evidence supplied by the above figures, Dr. Stark stated that "bachelorhood is more destructive to life than the most unwholesome trades, or than residence in an unwholesome house or district, where there has never been the most distant attempt at sanitary improvement of any kind." And this view has been very generally accepted, not only by the public, but by professed statisticians. Yet as a matter of fact, we believe that no such inferences can legitimately be drawn from the above table. Dr. Stark appears to us to have fallen into the mistake, which M. Quetelet tells us is so common, of trying to make his statistics carry more weight than they are capable of bearing. It is important that the matter should be put in a just light, for the Royal Commission on the Law of

Marriage has revealed no more striking fact than that of the prevalence of immature marriages, and such reasoning as Dr. Stark's certainly cannot tend to discourage these unwise alliances. If death strikes down in five years only half as many of those who are married as of those who are unmarried between the age of 20 and 25 (as appears from the above table), and if the proportion of deaths between the two classes goes on continually diminishing in each successive lustre (as is also shown by the above table), it seems reasonable to infer that the death-rate would be even more strikingly disproportionate in the case of persons between the ages of fifteen and twenty than in the case of persons between the ages of twenty and twenty-five. We believe, indeed, that if Dr. Stark had extended his table to include the former ages, the result would have been such as we have indicated. Yet few will suppose that such very youthful marriages can exercise so singularly beneficial an effect.

To many, Dr. Stark's conclusion may appear to be a natural and obvious *sequitur* from the evidence upon which it is founded. Admitting the facts — and we see no reason for doubting them — it may appear at first sight that we are bound to accept the conclusion that matrimony is favorable to longevity. Yet the consideration of a few parallel cases will suffice to show how small a foundation the figures we have quoted supply for such a conclusion. What would be thought, for example, of any of the following inferences? Among hot-house plants there are observed a greater variety and brilliancy of color than among those which are kept in the open air, therefore the housing of plants conduces to the splendor of their coloring. Or again: The average height of Life Guardsmen is greater than that of the rest of the male population, therefore to be a Life Guardsman conduces to tallness of stature. Or, to take an example still more closely illustrative of Dr. Stark's reasoning — the average longevity of noblemen exceeds that of untitled persons, therefore to have a title is conducive to longevity; or to borrow his words, "to remain without a title is more destructive to life than the most unwholesome trades, or than residence in an unwholesome house or district, where there has never been the most distant attempt at sanitary improvement of any kind."

We know that the inference is absurd in each of the above instances, and we are able at once to show where the flaw in the reasoning lies. We know that splendid flowers are more commonly selected for housing, and that Life Guardsmen are chosen for their tallness, so that we are prevented from falling into the mistake of ascribing splendor of color in the one instance, or tallness in the other, to the influence of causes which have nothing whatever to do with those attributes; nor is any one likely to ascribe the longevity of our nobility to the possession of a title. Yet there is nothing in any one of the above inferences which is in reality more unsound than Dr. Stark's inference from the mortality bills, when the latter are considered with due reference to the principles of interpretation which statisticians are bound to follow.

The fact is, that in dealing with statistics the utmost care is required in order that our inferences may not be pushed beyond the evidence afforded by our facts. In the present instance, we have simply to deal with the fact that the death-rate of unmarried men is higher than the death-rate of married men and widowers. From this fact we cannot reason as Dr. Stark has done to a *simple* conclusion. All that we can do is to show that one of *three* conclusions must be adopted: Either matrimony is favorable (directly or indirectly) to longevity, in a degree sufficient wholly to account for the observed peculiarity; or a principle of selection — the effect of which is such as, on the whole, to fill the ranks of married men from among the healthier and stronger portion of the community — operates in a sufficient degree to account wholly for the observed death-rates; or, lastly, the observed death-rates are due to the combination, in some unknown proportion, of the two causes just mentioned.

No reasonable doubt can exist, as it seems to us, that the third is the true conclusion to be drawn from the evidence supplied by the mortality bills. Unfortunately, the conclusion thus deduced is almost valueless, because we are left wholly in doubt as to the proportion which subsists between the effects to be ascribed to the two causes thus shown to be in operation. It scarcely required the evidence of statistics to prove that each cause must operate to some extent. It is perfectly obvious, on the one hand, that although hundreds of men who would be held by insurance companies to be "bad lives" may contract marriage, yet on the whole a principle of selection is in operation which must tend to bring the healthier portion of the male community into the ranks of the married, and to leave the unhealthier in the state of bachelorhood. A little consideration will show also that, on the whole, the members of the less healthy trades, very poor persons, habitual drunkards, and others whose prospects of long life are unfavorable, must (on the average of a large number) be more likely to remain unmarried than those more favorably situated. Another fact drawn from the Registrar-General's returns suffices to prove the influence of poverty on the marriage-rate. We refer to the fact that marriages are invariably more numerous in seasons of prosperity than at other times. Improvident marriages are undoubtedly numerous, but prosperity and adversity *have* their influence, and that influence not unimportant, on the marriage returns. On the other hand, it is perfectly obvious that the life of a married man is likely to be more favorable to longevity than that of a bachelor. The mere fact that a man has a wife and family depending upon him will suffice to render him more careful of his health, less ready to undertake dangerous employments, and so on; and there are other reasons which will occur to every one for considering the life of a married man better (in the sense of the insurance companies) than that of a bachelor. In fact, while we are compelled to reject Dr. Stark's statement that "bachelorhood is more destructive to life than the most unwholesome trades, or than residence in an unwholesome house or district, where there has never been the most distant attempt at sanitary improvement of any kind," we may safely accept his opinion that statistics "prove the truth of one of the first natural laws revealed to man—'It is not good that man should live alone.'" Whether the law required any proof is a question into which we need not enter.

THE TOPOGRAPHICAL SURVEY OF INDIA.

At the close of the war with Tippoo Sahib, Major Lambton planned the triangulation of the country lying between Madras and the Malabar coast, a district which had been roughly surveyed during the progress of the war by Colonel Mackenzie. The Duke of Wellington gave his approval to the project, and his brother, the Governor-General of India, and Lord Clive (son of the great Clive), Governor of Madras, used their influence to aid Major Lambton in carrying out his design. The only astronomical instrument made use of by the first survey-party was one of Ramsden's zenith-sectors, which Lord Macartney had placed in the hands of Dinwiddie, the astronomer, for sale. A steel chain, which had been sent with Lord Macartney's embassy to the Emperor of China and refused, was the only apparatus available for measuring.

Thus began the Great Trigonometrical Survey of India, a work whose importance it is hardly possible to over-estimate. Conducted successively by Colonel Lambton, Sir George Everest, Sir Andrew Waugh, and Lieutenant-Colonel Walker (the present superintendent), the trigonometrical survey has been prosecuted with a skill and accuracy which render it fairly comparable with the best works of European surveyors. But to complete in this style the survey of the whole of India would be the work of several centuries. The trigonometrical survey of Great Britain and Ireland has been already more than a century in progress, and is still unfinished. It can, therefore, be imagined that the survey of India — nearly ten times the size of the British Isles, and presenting difficulties a hundred-fold greater than those which the surveyor in England has to encounter — is not a work which can be quickly completed.

But the growing demands of the public service have rendered it imperatively necessary that India should be rapidly and completely surveyed. This necessity led to the commencement of the Topographical Survey of India, a work which has been pushed forward at a surprising rate during the past few years. Our readers may form some idea of the energy with which the survey is in progress from the fact that Colonel Thuillier's Report for the season 1866-'67 announces the charting of an area half as large as Scotland, and the preparatory triangulation of an additional area nearly half as large as England.

In a period of thirty years, with but few surveying parties at first, and a slow increase in their number, an area of 160,000 square miles has been completed and mapped by the topographical department. The revenue surveyors have also supplied good maps (on a similar scale) of 364,000 square miles of country during the twenty years ending in 1866. Combining these results, we have an area of 524,000 square miles, or upward of four times that of Great Britain and Ireland. For all this enormous area the surveyors have the records in a methodical and systematic form, fit for incorporation in the atlas of India. Nor does this estimate include the older revenue surveys of the northwestern provinces, which, for want of proper supervision in former years, were never regularly reduced. The records of these surveys were destroyed in the mutiny — chiefly in Hazaumbaugh and the southwestern frontier agency. The whole of these districts remain to be gone over in a style very superior to that of the last survey.

The extent of the country which has been charted may lead to the impression that the survey is little more than a hasty reconnoissance. This, however, is very far indeed from being the case. The preliminary triangulation, which is the basis of the topographical survey, is conducted with extreme care. In the present Report, for instance, we find that the discrepancies between the common sides of the triangles — in other words, the discrepancies between the results obtained by different observers — are in some cases less than one-tenth of an inch per mile; in others they are from one inch to a foot per mile; and in the survey of the Cossyah and Garrow Hills, where observations had to be taken to large objects such as trees, rocks, etc., with no defined points for guidance, the results differ by as much as twenty-six inches per mile. These discrepancies must not only be regarded as insignificant in themselves, but must appear yet more trifling when it is remembered that they are not cumulative, inasmuch as the preliminary triangulation is itself dependent on the great trigonometrical survey.

Let us understand clearly what are the various forms of survey which are or have been in progress in India. There are three forms to be considered: (1) The Great Trigonometrical Surveys; (2) The Revenue Surveys; and (3) the Topographical Surveys.

Great trigonometrical operations are extended in a straight course from one measured base to another. Every precaution which modern skill and science can suggest is taken in the measurement of each base-line, and in the various processes by which the survey is extended from one base-line to the other. The accuracy with which work of this sort is conducted may be estimated from the following instance: During the progress of the Ordnance Survey of Great Britain and Ireland, a base-line nearly eight miles long was measured near Loch Foyle in Ireland, and another nearly seven miles long on Salisbury Plain. Trigonometrical operations were then extended from Loch Foyle to Salisbury Plain, a distance of about 340 miles; and the Salisbury base-line was calculated from the observations made over this long arc. *The difference between the measured and calculated values of the base-line was less than five inches!* As we have stated, the trigonometrical survey of India will bear comparison with the best work of our surveyors in England.

A revenue survey is prosecuted for the definition of the boundaries of estates and properties. The operations of such a survey are therefore carried on conformably to those boundaries.

The topographical survey of a country is defined by Sir A. Scott Waugh to imply "the measurement and delineation of the natural features of a country, and the works of man thereon, with the object of producing a complete and sufficiently accurate map. Being free from the trammels of boundaries of properties, the principal lines of operations must conform to the features of the country, and objects to be surveyed."

The only safe basis for the topographical survey of a country is a system of accurate triangulation. And where the extent of country to be surveyed is large, there will always be a great risk of the accumulation of error in the triangulation itself; which must therefore be made to depend on the accurate results obtained by the great trigonometrical operations. In order to secure this result, fixed stations are established in the vicinity of the great trigonometrical series. Where this plan cannot be adopted, a net-work of large symmetrical triangles is thrown over the district to be surveyed, or boundary series of triangles are carried along the outline of the district or along convenient internal lines. The former of these methods is applicable to a hilly district, the latter to a flat country.

When the district to be surveyed has been triangulated, the work of filling-in the topographical details is commenced. Each triangle being of moderate extent, with sides from three to five miles in length, and the angular points being determined, as we have seen, with great exactness, it is evident that no considerable error can occur in filling-in the details. Hence, methods can be adopted in the final topographical work which would not be suitable for triangulation. The triangles can either be "measured up," or the observer may traverse from trigonometrical point to point, taking offsets and intersections; or, lastly, he may make use of the plane-table. The first two methods require little comment; but the principle of plane-tabling enters so largely into Indian surveying, that our notice would be incomplete without a brief account of this simple and beautiful method.

The plane-table is a flat board turning on a vertical pivot. It bears the chart on which the observer is planning the country. Suppose, now, that two points A and B are determined, and that we require to mark in the position of a third point C: It is clear that if we observed with a theodolite the angles A B C and B A C, we might lay these down on the chart with a protractor, and so the position of C would be determined with an accuracy proportioned to the care with which the observations were made, and the corresponding constructions applied to the chart. But in "plane-tabling" a more direct plan is adopted. A ruler bearing sights, resembling those of a rifle, is so applied that the edge passing through the point A of the chart (the observer being situated at the real station A) passes through the point B on the chart, the line of sight passing through the real station B. The table being fixed in the position thus obtained, the ruler is next directed so that its edge passes through A while the line of sight points to C. A line is now ruled with a pencil through A toward C. In a similar manner, the table having been removed to the station B, a pencil-line is drawn through the point B on the chart toward C. The two lines thus drawn determine by their intersection the place of C on the chart.

The above is only one instance of the modes in which a plane-table can be applied; there are several others. Usually the magnetic compass is made use of to fix the position of the table in accordance with the true bearing of the cardinal points. Also the bearings of several points are taken around each station; and thus a variety of tests of the correctness of the work become applicable. Into such points as these we need not here enter. It is sufficient that our readers should have been enabled to gather the simple principles on which plane-tabling depends, and the accuracy with which (when suitable precautions are taken) it can be applied as a method of observation subsidiary to the ordinary trigonometrical processes.

"A hilly country," says Sir A. Waugh, "offers the fairest field for the practice of plane-table surveys, and the more rugged the surface the greater will be the relative advantages and facilities this system possesses over the methods of actual measurement. On the other hand, in flat lands the plane-table works at a disadvantage, while the traverse system is facilitated. Consequently, in such tracts, the relative economy of the two systems does not offer so great a contrast as in the former. In closely-wooded or jungly tracts, all kinds of survey operations are prosecuted at a disadvantage; but in such localities, the commanding points must be previously cleared for trigonometrical operations, which facilitates the use of the table."

In whatever way the topographical details have been filled-in, a rigorous system of check must be applied to the work. The system adopted is that of running lines across ground that has been surveyed.

This is done by the head of the party or by the chief assistant-surveyor. A sufficient number of points are obtained in this way for comparison with the work of the detail surveyors; and when the discrepancies exceed certain limits, the work in which they appear is rejected. Owing to the extremely unhealthy, jungly, and rugged nature of the ground in which nearly all the Indian surveys have been progressing, it has not always been found practicable to check by regularly chained lines. There are, however, other modes of testing plane-table surveys, and as these entail less labor and expense in hilly and jungly tracts, and are quite as effective if thoroughly carried out, they have been adopted generally, while the measured routes or check-lines have only been pursued under more favorable conditions. Colonel Thuillier states that "the inspection of the work of every detailed surveyor in the field has been rigorously enforced, and the work of the field season is not considered satisfactory or complete unless this duty has been attended to."

The rules laid down to insure accuracy in the survey are — first, that the greatest possible number of fixed points should be determined by regular triangulation; secondly, that the greatest possible number of plane-table fixings should be made use of within each triangle; and, lastly, that eye-sketching should be reduced to a minimum. If these rules are well attended to, the surveyor can always rely on the value of the work performed by his subordinates. But all these conditions cannot be secured in many parts of the ground allotted to the several topographical parties, owing to the quantity of forest-land and the extremely rugged nature of the country. Hence arises the necessity for test-lines to verify the details or for some rigorous system of check; and this is more especially the case where native agency is employed.

So soon as the country has been accurately planned, the configuration of the ground has to be sketched up. This process is the end and aim of all the preceding work.

The first point attended to is the arterial system, or water-drainage, constituting the outfall of the country; whence are deduced the lines of greatest depression of the ground. Next the water-sheds or ridges of hills are traced in, giving the highest level. Lastly, the minor or subordinate features are drawn in with the utmost precision attainable. "The outlines of table-land should be well defined," says Sir A. Waugh, "and ranges of hills portrayed with fidelity, carefully representing the water-sheds or *divortia aquarum*, the spurs, peaks, depressions or saddles, isthmuses or connecting-links of separate ranges, and other ramifications. The depressed points and isthmuses are particularly valuable, as being either the sites of ordinary passes or points which new roads should conform to."

And here we must draw a distinction between survey and reconnoissance. It is absolutely necessary in making a survey that the outlines of ground as defined by ridges, water-courses, and feet of hills, should be rigorously fixed by actual observation and careful measurement. In reconnoitring, more is trusted to the eye.

The scale of the Indian topographical survey is that of one inch per mile; the scale of half an inch per mile being only resorted to in very densely-wooded or jungly country, containing few inhabitants and little cultivated, or where the climate is so dangerous that it is desirable to accelerate the progress of the survey.

On the scale of one inch per mile the practised draughtsman can survey about five square miles of average country per day. In intricate ground, intersected by ravines or covered by hills of irregular formation, the work proceeds much more slowly;

on the other hand, in open and nearly level country, or where the hills have simple outlines, the work will cost less and proceed more rapidly. On the scale of one inch per mile all natural features (such as ravines or water-courses) more than a quarter of a mile in length can be clearly represented. Villages, towns, and cities can be shown, with their principal streets and roads, and the outlines of fortifications. The general figure and extent of cultivated, waste, and forest lands, can be delineated with more or less precision, according to their extent. Irrigated rice-lands should be distinctly indicated, since they generally exhibit the contour of the ground.

The relative heights of hills and depths of valleys should be determined during the course of a topographical survey. These vertical elements of a survey can be ascertained by trigonometrical or by barometrical observations, or by a combination of both methods. "The barometer," says Sir A. Waugh, "is more especially useful for determining the level of low spots from which the principal trigonometrical stations are invisible. In using this instrument, however, in combination with the other operations, the relative differences of heights are to be considered the quantities sought, so that all the results may be referable to the original trigonometrical station. The height above the sea-level of all points coming under any of the following heads are especially to be determined, for the purpose of illustrating the physical relief of the country:

"1. The peaks and highest points of ranges.

"2. All obligatory points required for engineering works, such as roads, drainage, and irrigation, viz.: the highest points or necks of valleys; the lowest depressions or passes in ranges; the junctions of rivers, and *débouchements* of rivers from ranges; the height of inundation-level, at moderate intervals of about three miles apart.

"3. Principal towns or places of note."

Of the various methods employed to indicate the steepness of slope, that of eye-contouring seems alone to merit special comment. In true contouring, regular horizontal lines, at fixed vertical intervals, are traced over a country, and plotted on to the maps. This is an expensive and tedious process, whereas eye-contouring is easy, light, and effective. On this system all that is necessary is that the surveyor should consider what routes persons moving horizontally would pursue. He draws lines on his chart approximating as closely as possible to these imaginary lines. It is evident that when lines are thus drawn for different vertical elevations, the resulting shading will be dark or light, according as the slope is steep or gentle. This method of shading affords scope as well for surveying skill as for draughtsmanship.

A SHIP ATTACKED BY A SWORD-FISH.

We have always been puzzled to imagine how the "nine-and-twenty knights of fame," described in the "Lay of the Last Minstrel," managed to "drink the red wine through the helmet barred." But in Nature we meet with animals who seem almost as inconveniently armed as those chosen knights, who

> . . . "quitted not their armor bright,
> Neither by day, nor yet by night."

Among such animals the sword-fish must be recognized as one of the most uncomfortably-armed creatures in existence. The shark has to turn on his back before he can eat, and the attitude scarcely seems suggestive of a comfortable meal. But the sword-fish can hardly even by that arrangement get his awkwardly-projecting snout out of the way. Yet doubtless this feature, which seems so inconvenient, is of great value to Xiphias. In some way as yet unknown it enables him to get his living. Whether he first kills some one of his neighbors with this instrument, and then eats him at his leisure, or whether he plunges it deep into the larger sort of fish, and, attaching himself to them in this way, sucks nutriment from them while they are yet alive, is not known to naturalists. Certainly, he is fond of attacking whales, but this may result not so much from gastronomic tastes as from a natural antipathy — envy, perhaps, at their superior bulk. Unfortunately for himself, Xiphias, though cold-blooded, seems a somewhat warm-tempered animal; and, when he is angered, he makes a bull-like rush upon his foe, without always examining with due care whether he is likely to take any thing by his motion. And when he happens to select for attack a stalwart ship, and to plunge his horny beak through thirteen or fourteen inches of planking, with perhaps a stout copper sheathing outside it, he is apt to find some little difficulty in retreating. The affair usually ends by his leaving his sword embedded in the side of the ship. In fact, no instance has ever been recorded of a sword-fish recovering his weapon (if we may use the expression) after making a lunge of this sort. Last Wednesday the Court of Common Pleas — rather a strange place, by-the-by, for inquiring into the natural history of fishes — was engaged for several hours in trying to determine under what circumstances a sword-fish might be able to escape scot-free after thrusting his snout into the side of a ship. The gallant ship "Dreadnought," thoroughly repaired, and classed A 1 at Lloyd's, had been insured for £3,000 against all the risks of the seas. She sailed on March 10, 1864, from Colombo, for London. Three days later, the crew, while fishing, hooked a sword-fish. Xiphias, however, broke the line, and a few moments after leaped half out of the water, with the object, it should seem, of taking a look at his persecutor, the "Dreadnought." Probably he satisfied himself that the enemy was some abnormally large Cetacean, which it was his natural duty to attack forthwith. Be this as it may, the attack was made, and at four o'clock the next morning the captain was awakened with the unwelcome intelligence that the ship had sprung a leak. She was taken back to Colombo, and thence to Cochin, where she was hove down. Near the keel was found a round hole, an inch in diameter, running completely through the copper sheathing and planking.

As attacks by sword-fish are included among sea-risks, the insurance company was willing to pay the damages claimed by the owners of the ship, if only it could be proved that the hole had really been made by a sword-fish. No instance had ever been recorded in which a sword-fish had been able to withdraw his sword after attacking a ship. A defence was founded on the possibility that the hole had been made in some other way. Professor Owen and Mr. Frank Buckland gave their evidence; but neither of them could state quite positively whether a sword-fish which had passed its beak through three inches of stout planking could withdraw without the loss of its sword. Mr. Buckland said that fish have no power of "backing," and expressed his belief that he could hold a sword-fish by the beak; but then he admitted that the fish had considerable lateral power, and might so "wriggle its sword out of a hole." And so the insurance company will have to pay nearly six hundred pounds because an ill-tempered fish objected to be hooked, and took its revenge by running full tilt against copper-sheathing and oak-planking.

THE SAFETY-LAMP.

As THE late colliery explosions have attracted a considerable amount of attention to the principle of the safety-lamp, and questions have arisen respecting the extent of the immunity which the action of this lamp secures to the miner, it may be well for us briefly to point out the true qualities of the lamp.

In the Davy lamp a common oil-light is surrounded by a cylinder of wire-gauze. When the air around the lamp is pure the flame burns as usual, and the only effect of the gauze is somewhat to diminish the amount of light given out by the lamp. But so soon as the air becomes loaded with the carburetted hydrogen gas generated in the coal-strata, a change takes place. The flame grows larger and less luminous. The reason of the change is this: The flame is no longer fed by the oxygen of the air, but is surrounded by an atmosphere which is partly inflammable; and the inflammable part of the gas, so fast as it passes within the wire cylinder, is ignited and burns within the gauze. Thus the light now given out by the lamp is no longer that of the comparatively brilliant oil-flame, but is the light resulting from the combustion of carburetted hydrogen, or "fire-damp," as it is called; and every student of chemistry is aware that the flame of this gas has very little illuminating power.

So soon as the miner sees the flame thus enlarged and altered in appearance, he should retire. But it is not true that explosion would necessarily follow if he did not do so. The danger is great, because the flame within the lamp is in direct contact with the gauze, and if there is any defect in the wire-work, the heat may make for itself an opening which — though small — would yet suffice to enable the flame within the lamp to ignite the gas outside. So long, however, as the wire-gauze continues perfect, even though it become red hot, there will be no explosion. No authority is required to establish this point, which has been proved again and again by experiment; but we quote Professor Tyndall's words on the subject to remove some doubts which have been entertained on the matter: "Although a continuous explosive atmosphere," he says, "may extend from the air outside through the meshes of the gauze to the flame within, ignition is not propagated across the gauze. The lamp may be filled with an almost lightless flame; still explosion does not occur. A defect in the gauze, the destruction of the wire at any point by oxidation, hastened by the flame playing against it, would cause explosion;" and so on. It need hardly be said, however, that, imprudent as miners have often been, no miner would remain where his lamp burned with the enlarged flame indicative of the presence of fire-damp. The lamp should also be at once extinguished.

But here we touch on a danger which undoubtedly exists, and — so far as has yet been seen — cannot be guarded against by any amount of caution. Supposing the miner sought to extinguish the lamp by blowing it out, an explosion would almost certainly ensue, since the flame can be forced mechanically through the meshes, though it will not pass through them when it is burning in the ordinary way. Now, of course no miner who had been properly instructed in the use of the safety-lamp would commit such a mistake as this. But it happens, unfortunately, that sometimes the fire-damp itself forces the flame of the lamp through the meshes. The gas frequently issues with great force from cavities in the coal (in which it has been pent up), when the pick of the miner breaks an opening for it. In these circumstances an explosion is inevitable, if the issuing stream of gas happen to be directed full upon the lamp. Fortunately, however, this is a contingency which does not often arise. It is one of those risks of coal-mining which seem absolutely unavoidable by any amount of care or caution. It would be well if it were only such risks as these that the miner had to face.

Another peculiarity sometimes noticed when there is a discharge of fire-damp is worth mentioning. It happens, occasionally, that the light will be put out owing to the absolute exclusion of air from the lamp. This, however, can only happen when the gas issues in so large a volume that the atmosphere of the pit becomes irrespirable.

With the exception of the one risk which we have pointed out above, the Davy lamp may be said to be absolutely safe. It is necessary, however, that caution and intelligence should be exhibited in its use. On this point Professor Tyndall remarks that, unfortunately the requisite intelligence is not often possessed nor the requisite caution exercised by the miner, "and the consequence is that, even with the safety-lamp, explosions still occur." And he suggests that it would be well to exhibit to the miner in a series of experiments the properties of the valuable instrument which has been devised for his security. "Mere advice will not enforce caution," he says; "but let the miner have the physical image of what he is to expect clearly and vividly before his mind, and he will find it a restraining and monitory influence long after the effect of cautioning words has passed away."

A few words on the history of the invention may be acceptable. Early in the present century a series of terrible catastrophes in coal-mines had excited the sympathy of enlightened and humane persons throughout the country. In the year 1813, a society was formed at Sunderland to prevent accidents in coal-mines, or at least to diminish their frequency, and prizes were offered for the discovery of new methods of lighting and ventilating mines. Dr. William Reid Clanny, of Bishopwearmouth, presented to this society a lamp which burned without explosion in an atmosphere heavily loaded with fire-damp; for which invention the Society of Arts awarded him a gold medal. The Rev. Dr. Gray called the attention of Sir Humphry Davy to the subject, and that eminent chemist visited the coal-mines in 1815 with the object of determining what form of lamp would be best suited to meet the requirements of the coal-miners. He invented two forms of lamp before discovering the principle on which the present safety-lamps are constructed. This principle — the property, namely, that flame will not pass through small apertures — had been, we believe, discovered by Stephenson, the celebrated engineer, some time before; and a somewhat angry controversy took place respecting Davy's claim to the honor of having invented the safety-lamp. It seems admitted, however, by universal consent, that Davy's discovery of the property above referred to was made independently, and also that he was the first to suggest the idea of using wire-gauze in place of perforated tin.

In comparing the present frequency of colliery explosions with what took place before the invention of the safety-lamp, we must take into consideration the enormous increase in the coal-trade since the introduction of steam machinery. The number of miners now engaged in our coal-mines is far in excess of the number employed in the beginning of the present century. Thus accidents in the present day are at once more common on account of the increased rapidity with which the mines are worked, and when they occur there are more sufferers; so that the frequency of colliery explosions in the opening years of the present century, and the number of deaths resulting from them, are in reality much more significant than they seem to be at first sight. But even independently of this consideration, the record of the colliery accidents which took place at that time is sufficiently startling. Seventy-two persons were killed in a colliery at North Biddick at the commencement of the present century. Two explosions in 1805, at Hepburn and Oxclose, left no less than forty-three widows and a hundred and fifty-one children unprovided for. In 1808, ninety persons were killed in a coal-pit at Lumley. On May 24, 1812, ninety-one persons were killed by an explosion at Felling Colliery, near Gateshead. And many more such accidents might readily be enumerated.

THE DUST WE HAVE TO BREATHE.

A MICROSCOPIST, Mr. Dancer, F. R. A. S., has been examining the dust of our cities. The results are not pleasing. We had always recognized city dust as a nuisance, and had supposed that it derived the peculiar grittiness and flintiness of its structure from the constant macadamizing of city roads. But it now appears that the effects produced by dust, when, as is usual, it finds its way to our eyes, our nostrils, and our throats, are as nothing compared with the mischief it is calculated to produce in a more subtle manner. In every specimen examined by Mr. Dancer, animal life was abundant. But the amount of "molecular activity"— such is the euphuism under which what is exceedingly disagreeable to contemplate is spoken about — is variable according to the height at which the dust is collected. And of all heights which these molecular wretches could select for the display of their activity, the height of five feet is that which has been found to be the favorite. Just at the average height of the foot-passenger's mouth these moving organisms are always waiting to be devoured and to make us ill. And this is not all. As if animal abominations were insufficient, a large proportion of vegetable matter also disports itself in the light dust of our streets. The observations show that in thoroughfares where there are many animals engaged in the traffic, the greater part of the vegetable matter thus floating about "consists of what has passed through the stomachs of animals," or has suffered decomposition in some way or other. This unpleasing matter, like the "molecular activity," floats about at a height of five feet, or thereabouts.

After this, one begins to recognize the manner in which some diseases propagate themselves. What had been mysterious in the history of plagues and pestilences seems to receive at least a partial solution. Take cholera, for example. It has been shown by the clearest and most positive evidence that this disease is not propagated in any way save one — that is, by the actual swallowing of the cholera-poison. In Professor Thudichum's masterly paper on the subject in the *Monthly Microscopical Journal* it is stated that doctors have inhaled a full breathing from a person in the last stage of this terrible malady without any evil effects. Yet the minutest atom of the cholera-poison received into the stomach will cause an attack of cholera. A small quantity of this matter drying on the floor of the patient's room, and afterward caused to float about in the form of dust, would suffice to prostrate a houseful of people. We can understand, then, how matter might be flung into the streets, and, after drying, its dust be wafted through a whole district, causing the death of hundreds. One of the lessons to be learned from these interesting researches of Mr. Dancer is clearly this, that the watering-cart should be regarded as one of the most important of our hygienic institutions. Supplemented by careful scavengering, it might be effective in dispossessing many a terrible malady which now holds sway from time to time over our towns.

PHOTOGRAPHIC GHOSTS.

On the outskirts of the ever-widening circle lighted up by science there is always a border-land wherein superstition holds sway. The arts and sciences may drive away the vulgar hobgoblin of darker days; but they bring with them new sources of illusion. The ghosts of old could only gibber; the spirits of our day can read and write, and play on divers instruments, and quote Shakespeare and Milton. It is not, therefore, altogether surprising to learn that they can take photographs also. You go to have your photograph taken, we will suppose, desiring only to see your own features depicted in the *carte;* and lo! the spirits have been at work, and a photographic phantom makes its appearance beside you. It is true this phantom is of a hazy and dubious aspect — the "dull mechanic ghost" is indistinct, and may be taken for any one. Still, it is not difficult for the eye of fancy to trace in it the lineaments of some departed friend, who, it is to be assumed, has come to be photographed along with you. In fact, photography, according to the spiritualist, resembles what Byron called—

> "The lightning of the mind,
> Which, out of things familiar, undesigned,
> When least we deem of such, calls up to view
> The spectres whom no exorcism can bind."

The phenomena of spiritual photography were first observed some years since, and a set of carte photographs were sent from America to Dr. Walker, of Edinburgh, in which photographic phantoms were very obviously, however indistinctly, discernible. More recently an English photographer noticed a yet stranger circumstance, though he was too sensible to seek for a supernatural interpretation of it. When he took a photograph with a particular lens, there could be seen not only the usual portrait of the sitter, but at some little distance a faint "double," exactly resembling the principal image. Superstitious minds might find this result even more distressing than the phantom photographic friend. To be visited by the departed through the medium of a lens, is at least not more unpleasing than to hold converse with spirits through an ordinary "rapping" medium. But the appearance of a "double" or "fetch," has ever been held by the learned in ghostly lore to signify approaching death.

Fortunately, both one and the other appearance can be very easily accounted for without calling in the aid of the supernatural. At a recent meeting of the Photographical Society it was shown that an image may often be so deeply impressed on the glass that the subsequent cleaning of the plate, even with strong acids, will not completely remove the picture. When the plate is used for receiving another picture, the original image makes its reappearance, and as it is too faint to be recognizable, a highly-susceptible imagination may readily transform it into the image of a departed friend. The "double" is generated by the well-known property of double refraction, obtained by a lens under certain circumstances of unequal pressure, or sometimes by inequalities in the process of annealing. So vanish two ghosts which might have been more or less troublesome to those who are ready to see the supernatural in commonplace phenomena. Will the time ever come when no more such phantoms will remain to be exorcised?

THE OXFORD AND CAMBRIDGE ROWING STYLES.

WHATEVER opinion we may have of the result of the approaching boating contest, there can be no doubt that this year, as in former years, there is a striking dissimilarity between the rowing styles of the dark-blue and the light-blue oarsmen. This dissimilarity makes itself obvious whether we compare the two boats as seen from the side, or when the line of sight is directed along the length of either. Perhaps it is in the latter aspect that an unpractised eye will most readily detect the difference we are speaking of. Watch the Cambridge boat approaching you from some distance, or receding, and you will notice in the rise and fall of the oars, as so seen, the following peculiarities — a long stay of the oar in the water, a quick rise from and return to the water, the oars remaining out of the water for the briefest possible interval of time. In the case of the Oxford boat quite a different appearance is presented — there is a short stay in the water, a sharp rise from and return to it, and between these the oars appear to hang over the water for a perceptible interval. It is, however, when the boats are seen from the side that the meaning of these peculiarities is detected, and also that the fundamental distinction between the two styles is made apparent to the experienced eye. In the Cambridge boat we recognize the long stroke and "lightning feather" inculcated in the old treatise on rowing: in the Oxford boat we see these conditions reversed, and in their place the "waiting feather" and lightning stroke. By the "waiting feather," we do not refer to what is commonly understood by slow feathering, but to a momentary pause (scarcely to be detected when the crew is rowing hard) before the simultaneous dash of the oars upon the first grip of the stroke.* And observing more closely — which, by-the-way, is no easy matter — as either boat dashes swiftly past, we detect distinctive peculiarities of "work" by which the two styles are severally arrived at. In the Cambridge crew we see the first part of the stroke done with the shoulders — precisely according to the old-fashioned models — the arms straight until the body has fallen back to an almost upright position; then comes the sharp drop back of the shoulders beyond the perpendicular, the arms simultaneously doing their work, so that as the swing back is finished, the backs of the hands just touch the ribs in feathering. All these things are quite in accordance with what used to be considered the perfection of rowing; and, indeed, this style of rowing has some important good qualities and a very handsome appearance. The lightning feather, also, which follows the long sweeping stroke, is theoretically perfect. Now, in the case of the Oxford crew, we observe a style which at first sight seems less excellent. As soon as the oars are dashed down and catch their first hold of the water, the arms as well as the shoulders of each oarsman are at work.† The result is, that when the back has reached an upright position, the arms have already reached the chest, and the stroke is finished. Thus the Oxford stroke takes a perceptibly shorter time than the Cambridge stroke; it is also, necessarily, somewhat shorter in the water. One would, therefore, say it must be less effective. Especially would an unpractised observer form this opinion, because the Oxford stroke seems to be much shorter in range than it is in reality. *There* we have the secret of its efficiency. It is actually nearly as long as the Cambridge stroke, but is taken in a perceptibly shorter time. What does this mean but that the oar is taken much more sharply, and, therefore, much more effectively, through the water?

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* The grip is never properly caught without the pause: but any thing beyond a momentary pause is a bad fault in style.

† I write this with full knowledge that many Oxford men deny the fact. I have rowed behind Cambridge, Oxford, and London strokes, and have several times taken the place (number 2 thwart) of a London water-

Much more effectively so far as the actual conditions of the contest are concerned. The modern racing outrigger requires a sharp impulse, because it will take almost any speed we can apply to it. It will also retain that speed between the strokes, a consideration of great importance. The old-fashioned racing-eights required to be continually under propulsion. The lightning-feather was a necessity in their case, for between every stroke the boat would lag terribly with a slow-feathering crew We do not say; of course, that the speed of a light outrigged craft does not diminish between the strokes. Any one who has watched a closely-contested bumping-race, and noticed the way in which the sharply-cut bow of the pursuing boat draws up to the rudder of the other as by a succession of impulses, although either boat seen alone would seem to sweep on with almost uniform speed, will know that the motion of the lightest boat is not strictly uniform. But there is an immense difference between the almost imperceptible loss of way of a modern eight and the dead "lag" in the old-fashioned craft. And hence we get the following important consideration: Whereas with the old boats it was useless for a crew to attempt to give a very quick

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man in a four ("stroke" by John Mackinney) training for the Thames Regatta. So that I have had ample opportunities for comparing different rowing styles; and I am satisfied that the main defect of the real Cambridge style was (and perhaps is) an *exaggeration* of the sound rule that a boat should be propelled rather by the body than by the arms. The very swing in a Cambridge boat shows that this must be so. On the other hand, the Thames watermen do too much arm-work; and hence seem to double a little over their oars. I once rowed with some Cambridge friends from London nearly to Oxford and back, taking a Thames waterman as "help." We set him, at first, for our strokesman, but presently made him row *bow*, for we could none of us stand his gripping, arm-working style.

motion to their boat by a sharp, sudden "lift," this plan is calculated to be, of all others, the most effective with the modern racing-eight.

It may seem, at first sight, that, after all, the result of the Cambridge style should be as effective as that of the other. If arms and shoulders do their work in both crews with equal energy — which we may assume to be the case — and if the number of strokes per minute is equal, the actual propulsive energy ought to be equal likewise. A little consideration will show that this is a fallacy. If two men pull at a weight together they will move it farther with a given expenditure of energy than if first one and then the other apply his strength to the work. And, what is more to the purpose, they will be able to move it faster. So shoulders and arms working simultaneously will give a greater propulsive power than when working separately, even though in the latter case each works with its fullest energy. And not only so, but by the simultaneous use of arms and shoulders, that sharpness of motion can alone be given which is essential to the propulsion of a modern racing-boat.

We have said that the two crews are severally rowing in the style which has lately been peculiar to their respective universities. But the Cambridge crew is rowing in that form of the Cambridge style which brings it nearest to the requirements of modern racing. The faults of the style are subdued, so to speak, and its best qualities brought out effectually. In one or two of the long series of defeats lately sustained by Cambridge the reverse has been the case. At present, too, there is a certain roughness about the Oxford crew which encourages the hopes of the light-blue supporters. But it must be admitted that this roughness is rather apparent than real, great as it seems, and it will doubtless disappear before the day of encounter. We venture to predict that the "time" of the approaching race, taken in conjunction with the state of the tide, will show the present crews to be at least equal to the average.

BETTING ON HORSE-RACES: OR, THE STATE OF THE ODDS.

THERE appears every day in the newspapers an account of the betting on the principal forthcoming races. The betting on such races as the Two Thousand Guineas, the Derby, and the Oaks, often begins more than a year before the races are run; and during the interval, the odds laid against the different horses engaged in them vary repeatedly, in accordance with the reported progress of the animals in their training, or with what is learned respecting the intentions of their owners. Many who do not bet themselves, find an interest in watching the varying fortunes of the horses which are held by the initiated to be leading favorites, or to fall into the second rank, or merely to have an outside chance of success. It is amusing to notice, too, how frequently the final state of the odds is falsified by the event; how some "rank outsider" will run into the first place, while the leading favorites are not even "placed."

It is in reality a simple matter to understand the betting on races (or contests of any kind), yet it is astonishing how seldom those who do not actually bet upon races have any inkling of the meaning of those mysterious columns which indicate the opinion of the betting world respecting the probable results of approaching contests, equine or otherwise.

Let us take a few simple cases of "odds," to begin with; and, having mastered the elements of our subject, proceed to see how cases of greater complexity are to be dealt with.

Suppose the newspapers inform us that the betting is 2 to 1 against a certain horse for such and such a race, what inference are we to deduce? To learn this, let us conceive a case in which the *true* odds against a certain event are as 2 to 1. Suppose there are three balls in a bag, one being white, the others black. Then, if we draw a ball at random, it is clear that we are twice as likely to draw a black as to draw a white ball. This is technically expressed by saying that the odds are 2 to 1 *against* drawing a white ball; or 2 to 1 *on* (that is, in favor of) drawing a black ball. This being understood, it follows that, when the odds are said to be 2 to 1 against a certain horse, we are to infer that, in the opinion of those who have studied the performance of the horse, and compared it with that of the other horses engaged in the race, his chance of winning is equivalent to the chance of drawing one particular ball out of a bag of three balls.

Observe how this result is obtained: the odds are 2 to 1, and the chance of the horse is as that of drawing one ball out of a bag of three — three being the sum of the two numbers 2 and 1. This is the method followed in all such cases. Thus, if the odds against a horse are 7 to 1, we infer that the *cognoscenti* consider his chance equal to that of drawing one particular ball out of a bag of *eight*.

A similar treatment applies when the odds are not given as so many to *one*. Thus, if the odds against a horse are as 5 to 2, we infer that the horse's chance is equal to that of drawing a white ball out of a bag containing five black and two white balls — or seven in all.

We must notice also that the number of balls may be increased to any extent, provided the proportion between the total number and the number of a specified color remains unchanged. Thus, if the odds are 5 to 1 against a horse, his chance is assumed to be equivalent to that of drawing *one* white ball out of a bag containing six balls, only one of which is white; *or* to that of drawing a white ball out of a bag containing sixty balls, of which ten are white — and so on. This is a very important principle, as we shall now see.

Suppose there are two horses (among others) engaged in a race, and that the odds are 2 to 1 against one, and 4 to 1 against the other — what are the odds that one of the two horses will win the race? This case will doubtless remind our readers of an amusing sketch by Leech, called — if we remember rightly — "Signs of the Commission." Three or four undergraduates are at a "wine," discussing matters equine. One propounds to his neighbor the following question: "I say, Charley, if the odds are 2 to 1 against *Rataplan,* and 4 to 1 against *Quick March,* what's the betting about the pair?"—"Don't know, I'm sure," replies Charley; "but I'll give you 6 to 1 against them." The absurdity of the reply is, of course, very obvious; we see at once that the odds cannot be heavier against a pair of horses than against either singly. Still there are many who would not find it easy to give a correct reply to the question. What has been said above, however, will enable us at once to determine the just odds in this or any similar case. Thus — the odds against one horse being 2 to 1, his chance of winning is equal to that of drawing one white ball out of a bag of *three,* one only of which is white. In like manner, the chance of the second horse is equal to that of drawing one white ball out of a bag of *five,* one only of which is white. Now we have to find a number which is a multiple of both the numbers three and five. Fifteen is such a number. The chance of the first horse, modified according to the principle explained above, is equal to that of drawing a white ball out of a bag of fifteen of which *five* are white. In like manner, the chance of the second is equal to that of drawing a white ball out of a bag of fifteen of which *three* are white. Therefore, the chance that *one of the two* will win is equal to that of drawing a white ball out of a bag of fifteen balls of which *eight* (five added to three) are white. There remain seven black balls, and therefore the odds are 8 to 7 *on* the pair.

To impress the method of treating such cases on the mind of the reader, we take the betting about three horses — say 3 to 1, 7 to 2, and 9 to 1, *against* the three horses respectively. Then their respective chances are equal to the chance of drawing (1) one white ball out of *four,* one only of which is white; (2) a white ball out of *nine,* of which two only are white; and (3) one white ball out of *ten,* one only of which is white. The least number which contains four, nine, and ten, is 180; and the above chances, modified according to the principle explained above, become equal to the chance of drawing a white ball out of a bag containing 180 balls, when 45, 40, and 18 (respectively) are white. Therefore, the chance that one of the three will win is equal to that of drawing a white ball out of a bag containing 180 balls, of which 103 (the sum of 45, 40, and 18) are white. Therefore, the odds are 103 to 77 *on* the three.

One does not hear in practice of such odds as 103 to 77. But betting-men (whether or not they apply just principles of computation to such questions, is unknown to us) manage to run very near the truth. For instance, in such a case as the above, the odds on the three would probably be given as 4 to 3 — that is, instead of 103 to 77, or, which is the same thing, 412 to 308, the published odds would be 412 to 309.

And here a certain nicety in betting has to be mentioned. In running the eye down the list of odds, one will often meet such expressions as 10 to 1 against such a horse *offered*, or 10 to 1 *wanted*. Now, the odds of 10 to 1 *taken* may be understood to imply that the horse's chance is equivalent to that of drawing a certain ball out of a bag of eleven. But if the odds are offered and not taken, we cannot infer this. The offering of the odds implies that the horse's chance is *not better* than that above mentioned, but the fact that they are not taken implies that the horse's chance is *not so good*. If no higher odds are offered against the horse, we may infer that his chance is *very little worse* than that mentioned above. Similarly, if the odds of 10 to 1 are *asked for*, we infer that the horse's chance is *not worse* than that of drawing one ball out of eleven; if the odds are not obtained, we infer that his chance is *better;* and if no lower odds are asked for, we infer that his chance is *very little better.*

Thus, there might be *three* horses (A, B, and C) against whom the nominal odds were 10 to 1, and yet these horses might not be equally good favorites, because the odds might not be taken, or might be asked for in vain. We might accordingly find three such horses arranged thus:

	Odds.
A	10 to 1 (wanted).
B	10 to 1 (taken).
C	10 to 1 (offered).

Or these different stages might mark the upward or downward progress of the same horse in the betting. In fact, there are yet more delicate gradations, marked by such expressions respecting certain odds, as — *offered freely, offered, offered and taken* (meaning that some offers only have been accepted), *taken, taken and wanted, wanted,* and so on.

As an illustration of some of the principles we have been considering, let us take from the day's paper the state of the odds respecting the "Two Thousand Guineas." It is presented in the following form:

TWO THOUSAND GUINEAS.
7 to 2 against *Rosicrucian* (off.).
6 to 1 against *Pace* (off.; 7 to 1 w.).
10 to 1 against *Green Sleeve* (off.).
100 to 7 against *Blue Gown* (off.).
180 to 80 against Sir J. Hawley's lot (t.).

This table is interpreted thus: bettors are willing to lay the same odds against *Rosicrucian* as would be the true mathematical odds against drawing a white ball out of a bag containing two white and seven black balls; but no one is willing to back the horse at this rate. On the other hand, higher odds are not offered against him. Hence it is presumable that his chance is somewhat less than that above indicated. Again, bettors are willing to lay the same odds against *Pace* as might fairly be laid against drawing one white ball out of a bag of seven, one only of which is white; but backers of the horse consider that they ought to get the same odds as might be fairly laid against drawing the white ball when an additional black ball had been put into the bag. As respects *Green Sleeve* and *Blue Gown,* bettors are willing to lay the odds which there would be, respectively, against drawing a white ball out of a bag containing—(1) eleven balls, one only of which is white, and (2) one hundred and seven balls, seven only of which are white. Now, the three horses *Rosicrucian, Green Sleeve* and *Blue Gown,* all belong to Sir Joseph Hawley, so that the odds about the three are referred to in the last statement of the list just given. And since none of the offers against the three horses have been taken, we may expect the odds actually taken about "Sir Joseph Hawley's lot" to be more favorable than those obtained by summing up the three former in the manner we have already examined. It will be found that the resulting odds (offered) against Sir J. Hawley's lot — estimated in this way — should be, as nearly as possible, 132 to 80. We find, however, that the odds *taken* are 180 to 80. Hence, we learn that the offers against some or all of the three horses are considerably short of what backers require; or else, that some person has been induced to offer far heavier odds against Sir J. Hawley's lot than are justified by the fair odds against his horses, severally.

We have heard it asked why a horse is said to be a favorite, though the odds may be against him. This is very easily explained. Let us take as an illustration the case of a race in which four horses are engaged to run. If all these horses had an equal chance of winning, it is very clear that the case would correspond to that of a bag containing four balls of different colors; since, in this case, we should have an equal chance of drawing a ball of any assigned color. Now, the odds against drawing a particular ball would clearly be 3 to 1. This, then, should be the betting against each of the three horses. If any one of the horses has less odds offered against him, he is *a favorite*. There may be more than one of the four horses thus distinguished; and in that case, the horse against which the least odds are offered is *the first favorite*. Let us suppose there are two favorites, and that the odds against the leading favorite are 3 to 2, those against the other 2 to 1, and those against the best non-favorite 4 to 1; and let us compare the chance of the four horses. We have not named any odds against the fourth, because, if the odds against all the horses but one are given, the just odds against that one are determinable, as we shall see immediately. The chance of the leading favorite corresponds to the chance of drawing a ball out of a bag in which are three black and two white balls, *five* in all; that of the next to the chance of drawing a ball out of a bag in which are two black and one white ball, *three* in all; that of the third, to the chance of drawing a ball out of a bag in which are four black balls and one white one, *five* in all. We take, then, the least number containing both five and three — that is, *fifteen;* and then the number of white balls, corresponding to the chances of the three horses, are respectively six, five, and three, or fourteen in all; leaving only *one* to represent the chance of the fourth horse (against which the odds are, therefore, 14 to 1). Hence the chances of the four horses are respectively as the numbers *six*, *five*, *three*, and *one*.

We have spoken above of the published odds. The statements made in the daily papers commonly refer to wagers actually made, and therefore the uninitiated might suppose that every one who tried would be able to obtain the same odds. This is not the case. The wagers which are laid between practised betting-men afford very little indication of the prices which would be forced (so to speak) upon an inexperienced bettor. Book-makers — that is, men who make a series of bets upon several or all of the horses engaged in a race — naturally seek to give less favorable terms than the known chances of the different horses engaged would suffice to warrant. As they cannot offer such terms to the initiated, they offer them — and in general successfully — to the inexperienced.

It is often said that a man may so lay his wagers about a race as to make sure of gaining money whichever horse wins the race. This is not strictly the case. It is of course possible to make sure of winning if the bettor can only get persons to lay or take *the odds he requires to the amount he requires.* But this is precisely the problem which would remain insoluble if all bettors were equally experienced.

Suppose, for instance, that there are three horses engaged in a race with equal chances of success. It is readily shown that the odds are 2 to 1 against each. But if a bettor can get a person to take even betting against the first horse (A), a second person to do the like about the second horse (B), and a third to do the like about the third horse (C), and if all these bets are made to the same amount — say, £1,000 — then, inasmuch as only one horse can win, the bettor loses £1,000 on that horse (say A), and gains the same sum on each of the two horses B and C. Thus, on the whole he gains £1,000, the sum laid out against each horse.

If the layer of the odds had laid the true odds to the same amount on each horse, he would neither have gained nor lost. Suppose, for instance, that he laid £1,000 to £500 against each horse, and A won; then he would have to pay £1,000 to the backer of A, and to receive £500 from each of the backers of B and C. In like manner, a person who had backed each horse to the same extent would neither lose nor gain by the event. Nor would a backer or layer who had wagered *different* sums *necessarily* gain or lose by the race; he would gain or lose *according to the event.* This will at once be seen, on trial:

Let us next take the case of horses with unequal prospects of success — for instance, take the case of the four horses considered above, against which the odds were respectively 3 to 2, 2 to 1, 4 to 1, and 14 to 1. Here, suppose the same sum laid against each, and for convenience let this sum be £84 (because 84 contains the numbers 3, 2, 4, and 14). The layer of the odds wagers £84 to £56 against the leading favorite, £84 to £42 against the second horse, £84 to £21 against the third, and £84 to £6 against the fourth. Whichever horse wins, the layer has to pay £84; but if the favorite wins, he receives only £42 on one horse, £21 on another, and £6 on the third — that is £69 in all, so that he loses £15; if the second horse wins, he has to receive £56, £21, and £6 — or £83 in all, so that he loses £1; if the third horse wins, he receives £56, £42, and £6 — or £104, in all, and thus gains £20; and, lastly, if the fourth horse wins, he has to receive £56, £42, and £21 — or 119 in all, so that he gains £35. He clearly risks much less than he has a chance (however small) of gaining. It is also clear that in all such cases the worst event for the layer of the odds is, that the favorite should win. Accordingly, as professional book-makers are nearly always layers of odds, one often finds the success of a favorite spoken of in the papers as a "great blow for the bookmakers," while the success of a rank outsider will be described as "a misfortune to backers."

But there is another circumstance which tends to make the success of a favorite a blow to layers of the odds, and *vice versâ*. In the case we have supposed, the money actually pending about the four horses (that is, the sum of the amounts laid *for* and *against* them) was £140 as respects the favorite, £126 as respects the second, £105 as respects the third, and £90 as respects the fourth. But, as a matter of fact, the amounts pending about the favorites bear always a much greater proportion than the above to the amounts pending about outsiders. It is easy to see the effect of this. Suppose, for instance, that instead of the sums £84 to £56, £84 to £42, £84 to £21, and £84 to £6, a bookmaker had laid £8,400 to £5,600, £840 to £420, £84 to £21, and £14 to £1, respectively — then it will easily be seen that he would lose £7,958 by the success of the favorite; whereas he would gain £4,782 by the success of the second horse, £5,937 by that of the third, and £6,027 by that of the fourth. We have taken this as an extreme case; as a general rule, there is not so great a disparity as has been here assumed between the sums pending on favorites and outsiders.

Finally, it may be asked whether, in the case of horses having unequal chances, it is possible that wagers can be so proportioned (just odds being given and taken) that, as in the former case, a person backing, or laying against, all the four shall neither gain nor lose. It is so. All that is necessary is, that the sum actually pending about each horse shall be the same. Thus, in the preceding case, if the wagers £9 to £6, £10 to £5, £12 to £3, and £14 to £1, are either laid or taken by the same person, he will neither gain nor lose by the event, whatever it may be. And, therefore, if unfair odds are laid or taken about all the horses, in such a manner that the amounts pending on the several horses are equal (or nearly so), the unfair bettor must win by the result. Say, for instance, that instead of the above odds, he lays £8 to £6, £9 to £5, £11 to £3, and £13 to £1 against the four horses respectively; it will be found that he *must* win £1. Or if he *takes* the odds £18 to £11, £20 to £9, £24 to £5, and £28 to £1 (the just odds being £18 to £12, £20 to £10, £24 to £6, £28 to £2 respectively), he will win £1 by the race. So that, by giving or taking such odds to a sufficiently great amount, a bettor would be certain of pocketing a large sum, whatever the event of a given race might be.

In every instance, a man who bets on a race *must risk his money,* unless he can succeed in taking unfair advantages over those with whom he bets. Our readers will conceive how small must be the chance that an unpractised bettor will gain any thing but dearly-bought experience by speculating on horse-races. We would recommend those who are tempted to hold another opinion to follow the plan suggested by Thackeray in a similar case — to take *a good look* at professional and practised betting-men, and to decide "which of those men they are most likely to get the better of" in turf transactions.

SQUARING THE CIRCLE.

THERE must be a singular charm about insoluble problems, since there are never wanting persons who are willing to attack them. We doubt not that at this moment there are persons who are devoting their energies to Squaring the Circle, in the full belief that important advantages would accrue to science — and possibly a considerable pecuniary profit to themselves — if they could succeed in solving it. Quite recently, applications have been made to the Paris Academy of Sciences, to ascertain what was the amount which that body was authorized to pay over to any one who should square the circle. So seriously, indeed, was the secretary annoyed by applications of this sort, that it was found necessary to announce in the daily journals that not only was the Academy not authorized to pay any sum at all, but that it had determined never to give the least attention to those who fancied they had mastered the famous problem.

It is a singular circumstance that people have even attacked the problem without knowing exactly what its nature is. One ingenious workman, to whom the difficulty had been propounded, actually set to work to invent an arrangement for measuring the circumference of the circle; and was perfectly satisfied that he had thus solved a problem which had mastered all the mathematicians of ancient and modern times. That we may not fall into a similar error, let us clearly understand what it is that is required for the solution of the problem of "squaring the circle."

To begin with, we must note that the term "squaring the circle" is rather a misnomer; because the true problem to be solved is the determination of the length of a circle's circumference when the diameter is known. Of course, the solution of this problem, or, as it is termed, the *rectification* of the circle, involves the solution of the other, or the *quadrature* of the circle. But it is well to keep the simpler issue before us.

Many have supposed that there exists some exact relation between the circumference and the diameter of the circle, and that the problem to be solved is the determination of this relation. Suppose, for example, that the approximate relation discovered by Archimedes (who found, that if a circle's diameter is represented by *seven,* the circumference may be almost exactly represented by *twenty-two*) were strictly correct, and that Archimedes had proved it to be so: then, according to this view, he would have solved the great problem; and it is to determine a relation of some such sort that many persons have set themselves. Now, undoubtedly, if any relation of this sort could be established, the problem would be solved; but, as a matter of fact, no such relation exists, and the solution of the problem does not require that there should be any relation of the sort. For example, we do not look on the determination of the diagonal of a square (whose side is known) as an insoluble, or as otherwise than a very simple problem. Yet in this case no exact relation exists. We cannot possibly express both the side and the diagonal of a square in whole numbers, no matter what unit of measurement we adopt: or, to put the matter in another way, we cannot possibly divide both the side and the diagonal into equal parts (which shall be the same along each), no matter how small we take the parts. If we divide the side into 1,000 parts, there will be 1,414 such parts, *and a piece over,* in the diagonal; if we divide the side into 10,000 parts, there will be 14,142, and still a little piece over, in the diagonal; and so on for ever. Similarly, the mere fact that no exact relation exists between the diameter and the circumference of a circle is no bar whatever to the solution of the great problem.

Before leaving this part of the subject, however, we may mention a relation which is very easily remembered, and is very nearly exact — much more so, at any rate, than that of Archimedes. Write down the numbers 113355, that is, the first three odd numbers each repeated twice over. Then separate the six numbers into two sets of three, thus: 113)355, and proceed with the division thus indicated. The result, 3.1415929 . . . . expresses the circumference of a circle whose diameter is 1, correctly to the sixth decimal place, the true relation being 3.14159265 . . . .

Again, many people imagine that mathematicians are still in a state of uncertainty as to the relation which exists between the circumference and the diameter of the circle. If this were so, scientific societies might well hold out a reward to any one who could enlighten them; for the determination of this relation (with satisfactory exactitude) may be held to lie at the foundation of the whole of our modern system of mathematics. We need hardly say that no doubt whatever rests on the matter. A hundred different methods are known to mathematicians by which the circumference may be calculated from the diameter with any required degree of exactness. Here is a simple one, for example: Take any number of the fractions formed by putting *one* as the numerator over the successive numbers. Add together the alternate ones beginning with the first, which, of course, is unity. Add together the remainder. Subtract the second sum from the first. The remainder will express the circumference (the diameter being taken as unity) to any required degree of exactness. We have merely to take enough fractions. The process would, of course, be a very laborious one, if great exactness were required, and as a matter of fact, mathematicians have made use of much more convenient methods for determining the required relation; but the method is strictly exact.

The largest circle we have much to do with in scientific questions is the earth's equator. As a matter of curiosity, we may inquire what the circumference of the earth's orbit is; but as we are far from being sure of the exact length of the radius of that orbit (that is, of the earth's distance from the sun), it is clear that we do not need a very exact relation between the circumference and the diameter in dealing with that enormous circle. Confining ourselves, therefore, to the circle of the earth's equator, let us see what exactness we seem to require. We will suppose for a moment that it is possible to measure round the earth's equator without losing count of a single yard, and that we want to gather from our estimate what the diameter of this great circle may be. This seems, indeed, the only use to which, in this case, we can put our knowledge of the relation we are dealing with. We have then a circle some twenty-five thousand miles round, and each mile contains one thousand seven hundred and sixty yards; or, in all, there are some forty-four million yards in the circumference, and therefore (roughly) some fourteen million yards in the diameter of this great circle. Hence, if our relation is correct within a fourteen-millionth part of the diameter, or forty-four-millionth part of the circumference, we are safe from any error exceeding a yard. All we want, then, is that the number expressing the circumference (the diameter being unity) should be true to the eighth decimal place, as quoted above.

But, as we have said, mathematicians have not been content with a computation of this sort. They have calculated the number not to the *eighth,* but to the *six hundred and twentieth* decimal place. Now, if we remember that each new decimal makes the result ten times more exact, we shall begin to see what a waste of time there has been in this tremendous calculation. We all remember the story of the horse which had twenty-four nails in its shoes, and was valued at the sum obtained by adding together a farthing for the first nail, a halfpenny for the next, a penny for the next, and so on; doubling twenty-four times. The result was counted by thousands of pounds. The old miser who paid at a similar rate for a grave eighteen feet deep (doubling for each foot), killed himself when he heard the total. But now consider the effect of multiplying by ten, six hundred and twenty times. A fraction, with that enormous number for denominator, and unity for numerator, expresses the minuteness of the error which would result if the "long value" of the circumference were made use of. Let an illustration present the meaning of this:—

It has been estimated that light, which could eight times circle the earth in a second, takes 50,000 years in reaching us from the faintest star seen in Lord Rosse's giant reflector. Suppose we knew the exact length of the tremendous line which extends from the earth to such a star, and wanted, for some inconceivable purpose, to know the length of the circumference of a circle of which that line was the radius. The value deduced from the above-mentioned calculation of the relation between the circumference and the diameter would differ from the truth by a length which would be imperceptible under the most powerful microscope ever yet constructed. Nay, the radius we have conceived, enormous as it is, might be increased a million-fold, or a million times a million-fold, with the same result. And the area of the circle formed with this increased radius would be determinable with so much accuracy, that the error, if presented in the form of a minute square, would be utterly imperceptible under a microscope a million times more powerful than the best ever yet constructed by man.

Not only has the length of the circumference been calculated once in this unnecessarily exact manner, but a second calculator has gone over the work independently. The two results are of course identical figure for figure.

It will be asked then, what *is* the problem about which so great a work has been made? The problem is, in fact, utterly insignificant; its only interest lies in the fact that it is insoluble — a property which it shares along with many other problems, as the trisection of an angle, the duplication of a cube, and so on.

The problem is simply this: *Having given the diameter of a circle, to determine, by a geometrical construction, in which only straight lines and circles shall be made use of, the side of a square equal in area to the circle.* As we have said, the problem is solved, if, by a construction of the kind described, we can determine the length of the circumference; because then the rectangle under half this length and the radius is equal in area to the circle, and it is a simple problem to describe a square equal to a given rectangle.

To illustrate the kind of construction required, we give an approximate solution which is remarkably simple, and, so far as we are aware, not generally known. Describe a square about the given circle, touching it at the ends of two diameters, AOB, COD, at right angles to each other, and join CA; let COAE be one of the quarters of the circumscribing square, and from E draw E G, cutting off from AO a fourth part AG of its length, and from AC the portion AH. Then three sides of the circumscribing square together with AH are very nearly equal to the circumference of the circle. The difference is so small, that in a circle two feet in diameter, it would be less than the two-hundredth part of an inch. If this construction were exact, the great problem would have been solved.

One point, however, must be noted: the circle is of all curved lines the easiest to draw by mechanical means. But there are others which can be so drawn. And, if such curves as these be admitted as available, the problem of the quadrature of the circle can be readily solved. There is a curve, for instance, invented by Dinostratus which can readily be described mechanically, and has been called the quadratrix of Dinostratus, because it has the property of thus solving the problem we are dealing with.

As such curves can be described with quite as much accuracy as the circle — for, be it remembered, an absolutely perfect circle has never yet been drawn — we see that it is only the limitations which geometers have themselves invented that give this problem its difficulty. Its solution has, as we have said, no value; and no mathematician would ever think of wasting a moment over the problem — for this reason, simply, that it has long since been demonstrated to be insoluble by simple geometrical methods. So that, when a man says he has squared the circle (and many will say so, if one will only give them a hearing), he shows that either he wholly misunderstands the nature of the problem, or that his ignorance of mathematics has led him to mistake a faulty for a true solution.

THE NEW THEORY OF ACHILLES'S SHIELD.

A DISTINGUISHED classical authority has remarked that the description of Achilles's shield occupies an anomalous position in Homer's "Iliad." On the one hand, it is easy to show that the poem — for the description may be looked on as a complete poem — is out of place in the "Iliad"; on the other, it is no less easy to show that Homer has carefully led up to the description of the shield by a series of introductory events.

I propose to examine, briefly, the evidence on each of these points, and then to exhibit a theory respecting the shield which may appear *bizarre* enough on a first view, but which seems to me to be supported by satisfactory evidence.

An argument commonly urged against the genuineness of the "Shield of Achilles" is founded on the length and labored character of the description. Even Grote, whose theory is that Homer's original poem was not an *Iliad,* but an *Achilleis,* has admitted the force of this argument. He finds clear evidence that from Book II. to Book XX., Homer has been husbanding his resources for the more effective description of the final conflict. He therefore concedes the possibility that the "Shield of Achilles" may be an interpolation — perhaps the work of another hand.

It appears to me, however, that the mere length of the description is no argument against the genuineness of the passage. Events have, indeed, been hastening to a crisis up to the end of Book XVII., and the action is checked in a marked manner by the "Oplopœia" in Book XVIII. Yet it is quite in Homer's manner to introduce, between two series of important events, an interval of comparative inaction, or at least of events wholly different in character from those of either series. We have a marked instance of this in Books IX. and X. Here the appeal to Achilles and the night-adventure of Diomed and Ulysses are interposed between the first victory of the Trojans and the great struggle in which Patroclus is slain, and Agamemnon, Ulysses, Diomed, Machaon, and Eurypylus wounded.* In fact one cannot doubt that in such an arrangement Homer exhibits admirable taste and judgment. The contrast between action and inaction, or between the confused tumult of a heady conflict and the subtle advance of the two Greek heroes, is conceived in the true poetic spirit. The dignity and importance of the action, and the interest of the interposed events, are alike enhanced. Indeed, there is scarcely a noted author whose works do not afford instances of corresponding contrasts. How skilfully, for example, has Shakespeare interposed the "bald, disjointed chat" of the sleepy porter between the conscience-wrought horror of Duncan's murderers and the "horror, horror, horror" which "tongue nor heart could not conceive nor name" of his faithful followers. Nor will the reader need to be reminded of the frequent and effective use by Dickens of the contrast between the humorous and the pathetic.

The labored character of the description of the shield is an argument — though not, perhaps, a very striking one — of the independent origin of the poem.

But the arguments on which I am disposed to lay most stress lie nearer the surface.

Scarcely any one, I think, can have read the description of the shield without a feeling of wonder that Homer should describe the shield of a mortal hero as adorned with so many and such important objects. We find the sun and moon, the constellations, the waves of ocean, and a variety of other objects, better suited to adorn the temple of a great deity than the shield of a warrior, however noble and heroic. The objects depicted even on the Ægis of Zeus are much less important. There is certainly no trace in the "Iliad" of a wish on Homer's part to raise the dignity of mortal heroes at the expense of Zeus, yet the Ægis is thus succinctly described:

"Fringed round with ever-fighting snakes, though it was drawn to life
The miseries and deaths of fight; in it frowned bloody Strife,
In it shone sacred Fortitude, in it fell Pursuit flew,
In it the monster Gorgon's head, in which held out to view
Were all the dire ostents of Jove."—*Chapman's* Translation.

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* Another well-known instance, where "Patroclus sent in hot haste for news by a man of the most fiery impatience, is button-held by Nestor, and though he has no time to sit down, yet is obliged to endure a speech of 152 lines," is accounted for by Gladstone in a different manner.

Five lines here, as in the original, suffice for the description of Jove's Ægis, while one hundred and thirty lines are employed in the description of the celestial and terrestrial objects depicted on the shield of Achilles.

Another circumstance attracts notice in the description of Achilles's armor — the disproportionate importance attached to the shield. Undoubtedly, the shield was that portion of a hero's armor which admitted of the freest application of artistic skill. Yet this consideration is not sufficient to account for the fact that, while so many lines are given to the shield, the helmet, corselet, and greaves, are disposed of in four.

But the argument on which I am inclined to lay most stress is the occurrence *elsewhere* of a description which is undoubtedly only another version of the "Shield of Achilles." The "Shield of Hercules" occurs in a poem ascribed to Hesiod. But whatever opinion may be formed respecting the authorship of the description, there can be no doubt that it is not Hesiod's work. It exhibits no trace of his dry, didactic, somewhat heavy style. Elton ascribes the "Shield of Hercules" to an imitator of Homer, and in support of this view points out those respects in which the poem resembles, and those in which it is inferior to, the "Shield of Achilles." The two descriptions are, however, absolutely identical in many places; and this would certainly not have happened if one had been an honest imitation of the other. And those parts of the "Shield of Hercules" which have no counterparts in the "Shield of Achilles" are too well conceived and expressed to be ascribed to a very inferior poet — a poet so inferior as to be reduced to the necessity of simply reproducing Homer's words in other parts of the poem. Those parts which admit of comparison — where, for instance, the same objects are described, but in different terms — are certainly inferior in the "Shield of Hercules." The description is injured by the addition of unnecessary or inharmonious details. Elton speaks, accordingly, of these portions "as if they were expansions of the corresponding parts of the Shield of Achilles." This appears to me a mistake. It seems far more likely that both descriptions are by the same poet. It is not necessary for the support of my theory that this poet should be Homer, but I think both descriptions show undoubted traces of his handiwork. Indeed, all known imitations of Homer are so easily recognizable as the work of inferior poets, that I should have thought no doubt could exist on this point, but for the attention which the German theory respecting the "Iliad" has received. Assigning both poems to Homer, the "Shield of Hercules" may be regarded, not as an expansion (in parts) of the "Shield of Achilles," but as an earlier work of Homer's, improved and pruned by his maturer judgment, when he desired to fit it into the plan of the "Iliad." Or rather, each poem may be looked on as an abridgment (the "Shield of Hercules" the earlier) of an independent work on a subject presently to be mentioned.

It is next to be shown that, in the events preceding the "Oplopœia," there is a preparation for the introduction of a separate poem.

In the first place, every reader of Homer is familiar with the fact that the poet constantly make use, when occasion serves, of expressions, sentences, often even of complete passages, which have been already applied in a corresponding, or occasionally even in a wholly different, relation. The same epithets are repeatedly applied to the same deity or hero. A long message is delivered in the very words which have been already used by the sender of the message. In one well-known instance (in Book II.), not only is a message delivered thus, but the person who has received it repeats it to others in precisely the same terms. In the combat between Hector and Ajax (Book VI.), the flight of Ajax's spear, and the movement by which Hector avoids the missile, are described in six lines, differing only as to proper names from those which had been already used in describing the encounter between Paris and Menelaus (Book III.).

This peculiarity would be a decided blemish in a written poem. Tennyson, indeed, occasionally copies Homer's manner — for instance, in "Enid," he twice repeats the line —

"As careful robins eye the delver's toil;"

but with a good taste which prevents the repetition from becoming offensive. The fact is, that the peculiarity marks Homer as the *singer,* not the *writer,* of poetry. I would not be understood as accepting the theory, according to which the "Iliad" is a mere string of ballads. I imagine that no one who justly appreciates that noble poem would be willing to countenance such a theory. But that the whole poem was sung by Homer at those prolonged festivals which formed a characteristic peculiarity of Achaian manners seems shown, not only by what we learn respecting the later "rhapsodists," but by the internal evidence of the poem itself.*

Homer, reciting a long and elaborate poem of his own composition, occasionally varying the order of events, or adding new episodes, extemporized as the song proceeded, would exhibit the peculiarity invariably observed in the "improvisator," of using, more than once, expressions, sentences, or passages, which happened to be conveniently applicable. The art of extemporizing depends on the capacity for composing fresh matter while the tongue is engaged in the recital of matter already composed. Any one who has watched a clever improvisator cannot fail to have noticed that, though gesture is aptly wedded to words, the thoughts are elsewhere. In the case, therefore, of an improvisator, or even of a rhapsodist reciting from memory, the occasional recurrence of a well-worn form of words serves as a relief to the strained invention or memory.

We have reason, then, for supposing that if Homer had, in his earlier days, composed a poem which was applicable, with slight alterations, to the story of the "Iliad," he would endeavor, by a suitable arrangement of the plan of his narrative, to introduce the lines whose recital had long since become familiar to him.

Evidence of design in the introduction of the "Shield of Achilles" certainly does not seem wanting.

It is by no means necessary to the plot of the "Iliad" that Achilles should lose the celestial armor given to Peleus as a dowry with Thetis. On the contrary, Homer has gone out of his way to render the labors of Vulcan necessary. Patroclus has to be so ingeniously disposed of, that while the armor he had worn is seized by Hector, his body is rescued, as are also the horses and chariot of Achilles.

We have the additional improbability that the armor of the great Achilles should fit the inferior warriors Patroclus and

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* Besides Homer's reference, both in the "Iliad" and the "Odyssey," to poetic recitations at festivals, there is the well-known invocation in Book II. To what purpose would the mere writer of poetry pray for an increase of his physical powers? Nothing could be more proper, says Gladstone, if Homer were about to write; nothing less proper if he were engaged on a written poem.

Hector. Indeed, that the armor should fit Hector, or rather, that Hector should fit the armor, the aid of Zeus and Ares has to be called in:

> "To this Jove's sable brows did bow; and he made fit his limbs
> To those great arms, to fill which up the war-god entered him
> Austere and terrible, his joints and every part extends
> With strength and fortitude."—*Chapman's* Translation.

It is clear that the narrative would not have been impaired in any way, while its probability and consistency would have been increased, if Patroclus had fought in his own armor. The death of Patroclus would in any case have been a cause sufficient to arouse the wrath of Achilles against Hector — though certainly the hero's grief for his armor is nearly as poignant as his sorrow for his friend's death.

It appears probable, then, that the description of Achilles's Shield is an interpolation — the poet's own work, however, and brought in by him in the only way he found available. The description clearly refers to the same object which is described (here, also, only in part) in the "Shield of Hercules." The original description, doubtless, included all that is found in both "shields," and probably much more.

What, then, was the object to which the original description applied? An object, I should think, far more important than a warrior's shield. I imagine that any one who should read the description without being aware of its accepted interpretation would consider that the poet was dealing with an important series of religious sculptures, possibly that he was describing the dome of a temple adorned with celestial and terrestrial symbols.

In Egypt there are temples of a vast antiquity, having a dome, on which a zodiac — or, more correctly, a celestial hemisphere — is sculptured with constellation-figures. And we now learn, from ancient Babylonian and Assyrian sculptures, that these Egyptian zodiacs are in all probability merely copies (more or less perfect) of yet more ancient Chaldean zodiacs. One of these Babylonian sculptures is figured in Rawlinson's "Ancient Monarchies." It seems probable that in a country where Sabæanism, or star-worship, was the prevailing form of religion, yet more imposing proportions would be given to such zodiacs than in Egypt.

My theory, then, respecting the Shield of Achilles is this:

I conceive that Homer, in his Eastern travels, visited imposing temples devoted to astronomical observation and star-worship; and that nearly every line in both "shields" is borrowed from a poem in which he described a temple of this sort, its domed zodiac, and those illustrations of the labors of different seasons and of military or judicial procedures which the astrological proclivities of star-worshippers led them to associate with the different constellations.

I think there are arguments of some force to be urged in support of this theory, fanciful as it may seem.

In the first place, it is necessary that the constellations recognized in Homer's time (not necessarily, or probably, *by* Homer) should be distinguished from later inventions.

Aratus, writing long after Homer's date, mentions forty-five constellations. These were probably derived, without exception, from the globe of Eudoxus. Remembering the tendency which astronomers have shown, in all ages, to add to the list of constellations, we may assume that in Homer's time the number was smaller. Probably there were some fifteen northern and ten southern constellations, besides the twelve zodiacal signs. The smaller constellations mentioned by Aratus doubtless formed parts of larger figures. Any one who studies the heavens will recognize the fact that the larger constellations have been robbed of their just proportions to form the smaller asterisms. Corona Borealis was the right arm of Bootes, Ursa Minor was a wing of Draco (now wingless, and no longer a dragon), and so on.

Secondly, it is necessary that the actual appearance of the heavens, with reference to the position of the pole in Homer's time, should be indicated. For our present purpose, it is not necessary that we should know the exact date at which the most ancient of the zodiac-temples were constructed (or to which they were made to correspond). There are good reasons, though this is not the proper place for dwelling upon them, for supposing that the great epoch of reference among ancient astronomers preceded the Christian era by about 2,200 years. Be this as it may, any epoch between the date named and the probable date at which Homer flourished — say nine or ten centuries before the Christian era — will serve equally well for our present purpose. Now, if the effects of equinoctial precession be traced back to such a date, we are led to notice two singular and not uninteresting circumstances. First, the pole of the heavens fell in the central part of the great constellation Draco; and, secondly, the equator fell along the length of the great sea-serpent Hydra, in one part of its course, and elsewhere to the north of all the ancient aquatic constellations,* save that one-half of the northernmost fish (of the zodiac pair) lay north of the equator. Thus, if a celestial sphere were constructed with the equator in a horizontal position, the Dragon would be at the summit, Hydra would be extended horizontally along the equator — but with his head and neck reared above that circle — and Argo, Cetus, Capricornus, Piscis Australis, and Pisces — save one-half of the northernmost — would lie *below* the equator. It may, also, be mentioned that all the bird-constellations were then, as now, clustered together not far from the equator — Cygnus (the farthest from the equator) being ten degrees or so nearer to that circle than at present.

Now let us turn to the two "shields," and see whether there is anything in them to connect them with zodiac-temples, or to remind us of the relations exhibited above. To commence with the "Shield of Achilles," the opening lines inform us that there appeared —

> "The starry lights that heaven's high convex crowned,
> The Pleiads, Hyads, with the northern team,
> And great Orion's more refulgent beam."

And here, in Achilles's Shield, the list of constellations closes; but it is remarkable that in the "Shield of Hercules," while the above lines are wanting, we find lines which clearly point to other constellations. Remembering what has just been stated about Draco, it seems at the least a singular coincidence that we should find the centre or boss of the shield occupied by a dragon:

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* We may exclude Delphinus as probably later than Homer's time, though mentioned by Aratus.

> "The scaly horror of a dragon, coiled
> Full in the central field, unspeakable,
> With eyes oblique retorted, that aslant
> Shot gleaming flame."[*]—*Elton's* Translation.

We seem, also, to find a reference to the above-named relations of the aquatic constellations, and specially to the constellation Pisces:

> "In the midst,
> Full many dolphins chased the fry, and showed
> As though they swam the waters, to and fro
> Darting tumultuous: two[†] of silver scale
> Panting above the wave."

For we learn from both "shields" that the waves of ocean were figured in a position corresponding with the above-mentioned position of the celestial equator, beneath which—that is, *in the ocean,* on our assumption—the aquatic constellations were figured. The description of the ocean in the "Shield of Hercules" contains also some lines, in which we seem to see a reference to the bird-constellations close above the equator:

> "Rounding the utmost verge the ocean flowed
> As in full swell of waters, and the shield
> All variegated with whole circle bound.
> Swans of high-hovering wing there clamored shrill,
> Who also skimmed the breasted surge with plume
> Innumerous; near them fishes 'midst the waves
> Frolicked in wanton bounds."

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[*] Compare the description of the constellation Draco by Aratus:

> "Swol'n is his neck—eyes charged with sparkling fire
> His crested head illume. As if in ire
> To Helice he turns his foaming jaw
> And darts his tongue, barbed with a blazing star."
> *Lamb's* Translation.

[†] It is scarcely necessary to remark that no importance is to be attached to the numerical relations in this and other passages In the original work describing a zodiac-dome, the exact number of constellations representing fishes, dogs, or the like, would of course be mentioned; but any changes necessary to Homer's purpose in describing a shield would unhesitatingly have been introduced by him subsequently. It is singular, however, that we should have here, and in the passage quoted farther on as referring to Orion and the Dogs, the number *two* specially mentioned. The latter instance is the more remarkable, inasmuch as the mention of men and hares would lead one to expect that more than two dogs would be introduced. I would suggest as a sufficient reason for this peculiarity that the verbal alterations necessary to pluralize some of the objects in the dome would be more easily effected than those necessary to undualize others.

In the "Shield of Achilles" no mention is made of Perseus, but in the "Shield of Hercules" this well-known constellation seems described in the lines —

"There was the knight of fair-haired Danae born,
Perseus; nor yet the buckler with his feet
Touched nor yet distant hovered, strange to see,
For nowhere on the surface of the shield
He rested; so the crippled artist-god
Illustrious framed him with his hands in gold.
Bound to his feet were sandals winged; a sword
Of brass, with hilt of sable ebony,
Hung round him from the shoulders by a thong.
. . . . The visage grim
Of monstrous Gorgon all his back o'erspread;
. . . . the dreadful helm
Of Pluto clasped the temples of the prince."

I think that one may recognize a reference to the twins Castor and Pollux (the wrestler and boxer of mythology) in the words —

"But in another part
Were men who wrestled, or in gymnic fight
Wielded the cestus."

Orion is not mentioned by name in the "Shield of Hercules," as in the other; but Orion, Lepus, and the two dogs, seem referred to:

"Elsewhere men of chase
Were taking the fleet hares: two keen-toothed dogs
Bounded beside, these ardent in pursuit,
Those with like ardor doubling in their flight."

In each "shield" we find a reference to the operations of the year — hunting and pasturing, sowing, ploughing, and harvesting. It is hardly necessary to point out the connection between these operations and astronomical relations. That this connection was fully recognized in ancient times is shown in the "Works and Days" of Hesiod. We find also in Egyptian zodiacs clear evidence that these operations, as well as astronomical symbols or constellations, found a record in sculptured domes.

The judicial, military, and other proceedings described in the "Shield of Achilles" were also supposed by the ancients to have been influenced by the courses of the stars.

If we had no evidence that ancient celestial spheres presented the constellations above referred to, we might be disposed to attach less weight to the coincidences here presented; but the "Phenomena" of Aratus affords sufficient testimony on this point. In the first place, that work is of great antiquity, since Aratus flourished two centuries and a half before the Christian era; but it is well known that Aratus did not describe the results of his own observations. The positions of the constellations, as recorded by him, accord neither with the date at which he wrote nor with the latitude in which he lived. It is generally assumed — chiefly on the authority of Hipparchus — that Aratus borrowed his knowledge of astronomy from the sphere of Eudoxus; but we must go much farther back even than the date of Eudoxus, before we can find any correspondence between the appearance of the heavens and the description given by Aratus. Thus we may very fairly assume that the *origin* of the constellations (as distinguished from their association with certain circles of the celestial sphere) may be placed at a date preceding, perhaps by many generations, that at which Homer flourished.

Indeed, there have not been wanting those who find in the ancient constellations the record of the early history of man. According to their views, Orion is Nimrod — the "Giant," as the Arabic name of the constellation implies — the mighty hunter, as the dogs and hare beside him signify. The Centaur bearing a victim toward the altar is Noah; Argo, the stern of a ship, is the ark, as of old it might be seen on Mount Ararat. Corvus is the crow sent forth by Noah, and the bird is placed on Hydra's back to show that there was no land on which it could set its foot. The figure now called Hercules, but of old Engonasin, or the kneeler, and described by Aratus as "a man doomed to labor," is Adam. His left foot treads on the dragon's head, in token of the saying, "It shall bruise thy head;" and Serpentarius, or the serpent-bearer, is the promised seed.

Of course, if we accept these views, we have no difficulty in understanding that a poet so ancient as Homer should refer to the constellations which still appear upon celestial spheres.

And, in any case, the mere question of antiquity presents, as we have already shown, little difficulty.

But there is a difficulty in one respect, a notice of which must close this paper, already carried far beyond the limits I had proposed to myself: It may be thought remarkable that heroes of Greek mythology, as Perseus and Orion, should be placed by Homer, or even by Aratus, in spheres which are undoubtedly of Eastern origin.

Now, it may be remarked, first, of Homer, that many acute critics consider the whole story of the "Iliad" to be, in reality, merely an adaptation of an Eastern narrative to Greek scenes and names. It is pointed out that, whereas the Catalogue in Book II. reckons upward of 100,000 men, only 10,000 fought at Marathon; and, whereas there are counted no less than 1,200 ships in the Catalogue, there were but 271 at Artemisium, and at Salamis but 378. However this may be, we have the distinct evidence of Herodotus that the Greek mythology was derived originally from foreign sources. He says, "All the names of the gods in Greece were brought from Egypt," an opinion in which Diodorus and other eminent authorities concur. But it is the opinion of acute modern critics that we must go beyond Egyptian — to Assyrian, or Indian, perhaps even to Hebrew sources, for the origin of Greek mythology. Bryant traces nearly all the Greek myths to traditions of the dispersion of the Cuthites or Cuseans. And Layard has ascribed to Niebuhr the following significant remarks: "There is a want in Grecian art which neither I, nor any man now alive, can supply. There is not enough in Egypt to account for the peculiar art and the peculiar mythology which we find in Greece. That the Egyptians did not originate it I am convinced, though neither I, nor any man now alive, can say who were the originators. But the time will come when, on the borders of the Tigris and Euphrates, those who come after me will live to see the origin of Grecian art and Grecian mythology."

THE END.